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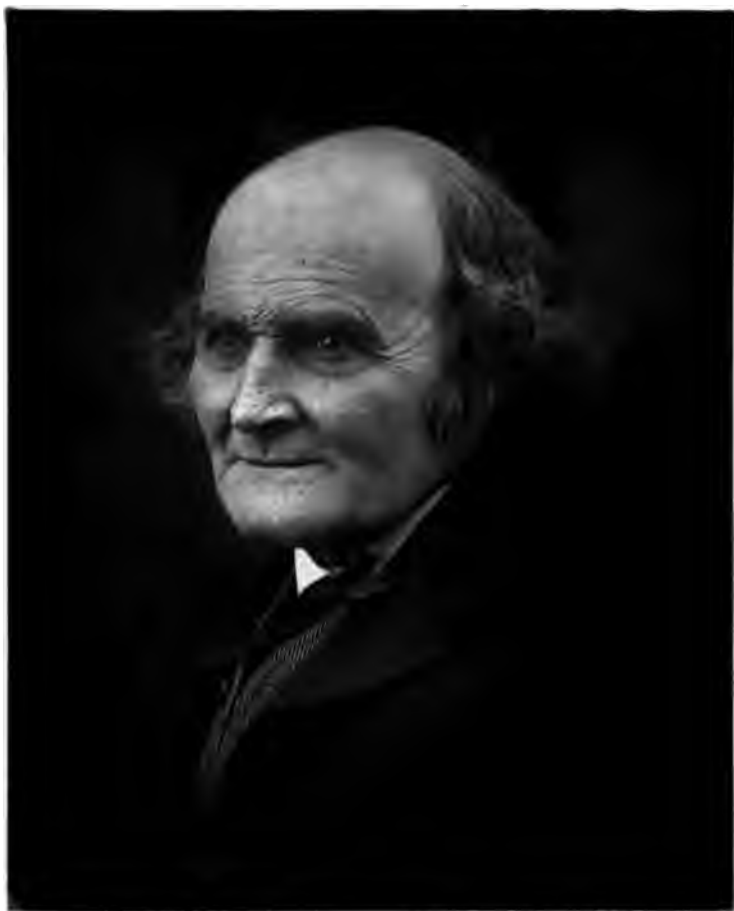
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A. Cayley

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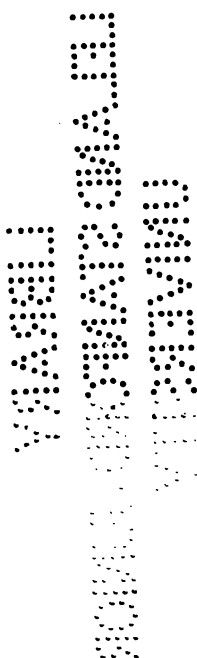
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PROCEEDINGS
OF
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“A Research into the Elasticity of the Living Brain and the Conditions governing the Recovery of the Brain after Compression for short Periods.”\* By A. G. LEVY, M.B. (London). Communicated by Professor V. HORSLEY, F.R.S. Received February 21,—Read March 15, 1894.

(From the Laboratory of the Pathological Department of University College, London.)

*A. Object and Method of the Research.*

1. *Introduction.*—This piece of work was undertaken at the suggestion of Professor V. Horsley, who devised the apparatus employed, and to whom I am indebted for advice and suggestions.

In view of the great frequency of compression of the brain as a pathological condition, it seems very advisable to attempt to obtain knowledge of some of the elementary factors conditioning the physical changes in the brain substance due to mechanical pressure.

The general plan adopted was (*a*) to compress the living brain (*i.e.*, the brain substance plus lymph and circulating blood) by a given weight for a given time, to measure the amount of direct compression, and, after removal of the pressure, to measure the elastic recoil; (*b*) to investigate the influence upon the recoil of varying conditions of the vascular and respiratory systems.

Finding in an early stage of the investigation that the elasticity of the brain is very far from being perfect, I was further led to investigate the conditions, vascular, &c., which favoured more complete recovery of the brain surface to its normal level, especially after periods of prolonged compression.

The following, therefore, are records of an investigation of the

\* Towards the expenses of this research a grant was made by the British Medical Association on the recommendation of the Scientific Grants Committee of the Association.

physical properties of the brain substance, and deductions drawn from my observations must not be immediately applied to explain the changes in the brain seen in cases of meningeal hæmorrhage, tumours, &c., because, in order to ascertain the fundamental facts, the experiments in the present case have of necessity been made with the cranial cavity open, the dura mater partially reflected, and a certain quantity of the cerebro-spinal fluid consequently drained away. In pathological conditions the pressure is exerted in a closed, fluid-containing cavity, and hence any local compression must evoke a reaction from all parts of the cerebral environments, and all parts of the intracranial contents are at an equally exaggerated pressure. In my experiments, the cranial cavity being opened, the influence of the cranial walls is limited, and the pressure is directly exerted upon, and principally confined to, the part of the surface compressed and the mass below it.

I may mention that I started with the generally accepted premise that the elasticity of the brain is more or less proportional to the central blood pressure, and many of my earlier experiments were performed with the object of testing what at first sight seems an exceedingly reasonable view.

2. *Historical Introduction.*—I have throughout my experiments taken as an indisputable fact what was demonstrated by Grashey,\* viz., that the cerebral tissue itself is practically incompressible (in fact, its coefficient of compressibility being rather less than that of water), and that any reduction in volume of the living brain must therefore be due to the squeezing out from its blood vessels, lymphatics, and ventricles, of blood, lymph, and cerebro-spinal fluid.

Grashey's experiments consisted of a careful estimation of the compressibility of the dead brain by hydraulic pressure in a delicate piezometer. His experiments conclusively showed what had indeed been the general teaching, but he was led to make this demonstration by reason of the extraordinary assertion of Adamkiewicz† that the brain tissue itself bears the brunt of the diminution of the intracranial space.

The records on compression of the brain substance are numerous, and the methods employed by the investigators (Cooper, Duret, Kahler, Pick, Leyden, Adamkiewicz, Spencer and Horsley, Dean, Hill, &c.) are very various; but these experiments have been arranged from the clinical point of view, *i.e.*, the effect of compression upon the functional activity of the nerve centres.

Some observers have noted the structural changes produced; thus,

\* Grashey, "Ueber Hirndruck und Hirncompression," *Allg. Ztschr. f. Psychiat.*, Berlin, 1887, 43.

† Adamkiewicz, "Die Lehre von Hirndruck und die Pathologie der Hirncompression," *Sitzungsberichte d. K. Akademie, Wien*, Band 88, 1883.

Adamkiewicz investigated the structures microscopically, and Dean\* by quantitative analysis, with special reference to œdema.

I can find, however, no systematic investigation into the immediate recovery of the brain after compression. Dean, in the course of his experiments, gave attention to the recovery after the compression by glass discs, but his observations were made by the unaided eye. Further, the majority of his experiments, in which the brain was examined several days after compression was removed, when secondary changes had taken place, fall into another category than those which I have carried out, for in no case were my observations prolonged for more than an hour after compression, and usually the period was much shorter. Moreover, contrary to the methods adopted by the just named observers, the influence of the cerebro-spinal fluid in the present research was excluded; the dura mater being opened, the fluid is released from all tension, and some of it escapes during the procedure described below, my object being to investigate the change in the brain mass alone.

3. *Description of the Apparatus for Measuring the Compression and Recovery therefrom.*—An apparatus was designed for the purpose of applying known weights vertically to the brain surface, so that the amount of depression of the surface could be accurately measured, and also the recoil after the removal of the weight.

This apparatus is shown in front and back view in figs. 1 and 2. It consists essentially of two supports for screwing into the skull, a plunger through the medium of which to apply the weights, and a dial to indicate the excursion of the plunger.

To consider the parts in detail: the supporting pillars consist of three portions, see fig. 1 (*a*, *b*, and *c*). The foot (*a*) screws into the bone, and (*b*) is a cap which screws on to it. The two when fitted together form a socket to receive the globular end of (*c*) the pillar, which thus acts in a ball-and-socket joint, and can be firmly fixed in any position by screwing down (*b*). By this means the pillar may be made to assume a perpendicular position when the screw has to be inserted into a portion of the skull which is not horizontal.

The pillars support the dial through the medium of the slots (*d*, *d*), and are fixed by screws (*e*, *e*), the slots themselves being firmly fixed in position by the thumb screws (*f*, *f*). These slots allow, among other movements, of lateral shifting of the dial. These arrangements conveniently give great range of movement of the dial, so that the plunger may be placed into a position of accurate perpendicularity.

The plunger consists of a straight rod, carrying above a weight pan (*g*), and, passing through the body of the dial, it has attached to it by a special arrangement a thread which, looping round the axis

\* H. P. Dean, "Cerebro-spinal Pressure," 'Journal of Pathology and Bacteriology,' 1892.

FIG. 1.

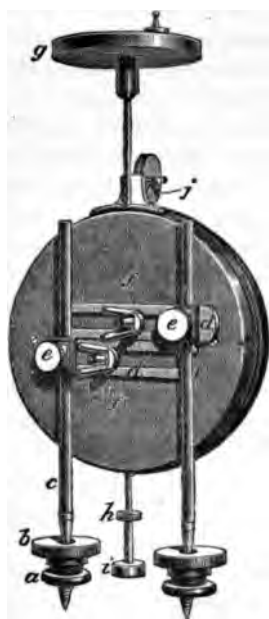


FIG. 2.



of the dial, rotates this in its excursions up and down. An index hand attached to the axis indicates tenths of millimetres on the dial.

Below, the plunger has screw arrangements (*h*), by which discs (*i*) of varying sizes can be applied. In all experiments a disc of 1 cm. diameter was used unless otherwise mentioned, and it was by the screw attachment correctly adjusted to the surface of the brain. The weight of the plunger was accurately counterbalanced by a weight which acts over a pulley (*j*), so that the disc remains at rest in any position, *e.g.*, on the brain surface, without exerting any pressure when there is no weight in the pan.

This suspended counterbalancing weight also acted as a plumb line by which the perpendicularity of the plunger was secured, this position being essential to avoid all friction. The figures show the apparatus applied to a horizontal surface.

4. *Method of Experiment.*—The animal, invariably a dog, was kept under ether through the medium of a glass cannula, inserted into the trachea and attached by india-rubber tubing to a funnel, this latter being placed over cotton-wool saturated with ether. The narcosis must be maintained equably throughout the experiment, to avoid irregularity in respiratory movements. The head was firmly sup-

ported in the usual holder. The skull was exposed on one side, and the median and fronto-temporal ridges chiselled away as far as necessary, so as to give a flat surface for inserting the screws. The skull was trephined close to, but carefully avoiding, the longitudinal sinus. I found hæmorrhage a very troublesome accident, and that it was essential to stop it by means of wax and amadou before proceeding with the experiment, as the presence of even a thin layer of blood on the cortex marred the result. The object of applying the compression as near as possible to the middle line was to arrange the line of pressure perpendicularly downwards, so as to avoid any elastic reaction from the falx cerebri; this seemed to be more important in using large weights, which evoke larger excursions. I enlarged the opening in the skull with bone forceps, so as to allow of easy insertion of the plunger, and give sufficient margin to observe the condition of the brain surface. The next step was to reflect the dura mater, insert the screws (using a guarded bradawl to make preliminary holes), and then to erect the apparatus so that the disc of the plunger rested flat on the surface of the brain, and the plunger itself was absolutely perpendicular.

The blood pressure was recorded by means of a mercurial manometer connected with the femoral artery. The respirations were traced by means of a Paul Bert receiver and a Marey recording tambour.

In performing an experiment I noted the moment of applying the weight to the pan, and then recorded the figures the index pointed to at intervals of so many seconds during and after compression, and also read off the blood pressure at stated intervals.

5. *Considerations Affecting the Plan of Experiments.*—A few remarks on special conditions observed during my experiments may now be briefly noted.

(a.) Various conditions of the cranium and its contents, affecting the apparent elasticity of the brain, and requiring attention, are as follows:—

*Adhesions* of the pia mater to the dura mater, as, for instance, by vessels running from one to the other.

*The Falx Cerebri.*—This is not large as a rule in the dog, often not more than 5 or 6 mm. broad in a small dog, and is not likely to have much influence, but large weights applied in a direction fairly perpendicularly to its surface might reasonably be expected to be limited in effect by its elasticity.

*The Tentorium.*—This is generally almost totally ossified in dogs, but in some dogs there exists sufficient membrane to prove a factor in causing some elastic reaction from the base of the skull.

*The Shape of the Cranium.*—It seems probable that the vertical depth of the skull cavity should modify results somewhat, as with a

6. Mr. A. G. Levy. *On the Elasticity of the Living*

deep skull a greater depth of brain substance comes directly under compression than in the case of a flat skull, but this and the foregoing conditions have but a subsidiary influence upon the effects observed, the main factor which influences the more immediate recoil of the brain surface, in experiments performed upon the organ *in situ*, being the rigidity of the bony floor it rests upon.

(b.) Moulding.—After a compression of thirty seconds or so there frequently results a faint ringed impression of the disc—this usually disappearing in about half an hour. After several experiments have been performed upon the same spot, a considerable saucer-like depression in some cases ensues.

(c.) Œdema.—On several occasions a curious phenomenon was observed, which most probably has its origin in local œdema. It is more especially marked in experimenting with small weights, 20 to 50 grams, applied for a couple of seconds only. When the weight, after compression, has been removed, and the brain surface, in recovering, has ceased to push the disc up farther, then if the passively resting disc be removed from the surface, the latter will start a further rise almost immediately, so that in a few minutes it may even be at a higher level than that at which it stood when the experiment was started. After more severe compression this is not observed.

(d.) Powerful respiratory movements, as will be shown directly, are liable to vitiate results by aiding recovery after compression, through the medium of increased venous pressure. Hence the necessity already mentioned of maintaining a fairly deep narcosis.

(e.) The direct effect of compression upon the blood pressure and respiration.

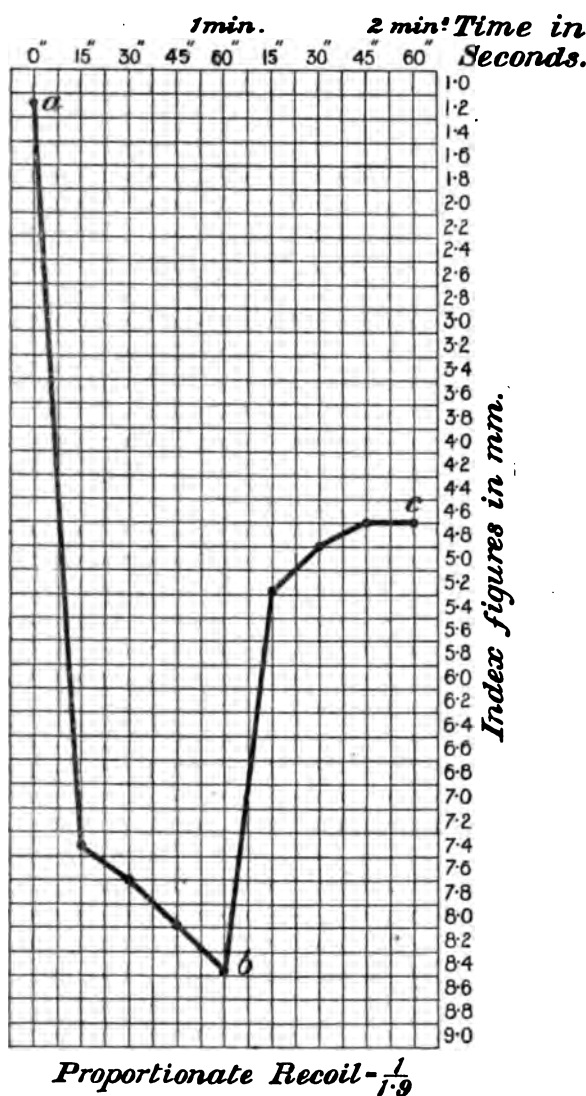
I found that the local compression by any weights that I employed was, in view of the cranium being freely opened, insufficient to produce any effect, either upon the blood pressure or respiration, this being so far fortunate as then could be no indirect influence from this cause upon the elasticity of the brain. The minimum weight which can affect respiration, even slightly, was 200 grams.

*B. Results of Compression Experiments on the uninjured Brain, the Blood Pressure being normal.*

(a.) *Definitions.*—I employ the term “uninjured brain” to denote that no previous experiment has been performed on the same brain; the effect of previous compression in destroying the value of subsequent experiments is discussed later.

The term “excursion” I have applied to the extent to which the plunger enters into the cranial cavity (by a reason of sinking of the brain) on a weight being applied. Fig. 3 (*ab*).

FIG. 3.



By the term "recoil" I mean the amount of recovery of the brain as measured by the instrument. Fig. 3 (bc).

(b.) *Results of the Experiments.*—In this section, unless otherwise stated, the weight used was 50 grams and the compressing disc 1 cm. in diameter.

i. *Excursion*.—The character of the excursion evoked by a compression of 50 grams for one minute is as follows :—

Firstly, there is a rapid fall of the index in the first few seconds ; this being followed by a very much more gradual compression (fig. 3). So that three-fourths of the whole excursion may take place in the first two seconds.

The absolute extent of the excursion varies in different dogs very considerably (the time of compression being equal) ; thus the amount in one minute varied in two separate cases from 4.5 mm. to 7.3 mm. Variation in the absolute amount of depression of the brain surface is to be expected where differently shaped heads were experimented upon.

There appeared to be some relation between the size of the dog and the extent of the excursion, probably because there is in a large brain a greater depth of collapsible mass beneath the plunger. On the other hand, it was very noticeable that the extent bears no relation to the height of the blood pressure ; in fact, it is curious that often the largest excursions accompanied a normally high blood pressure (i.e., a well-filled, highly collapsible brain).

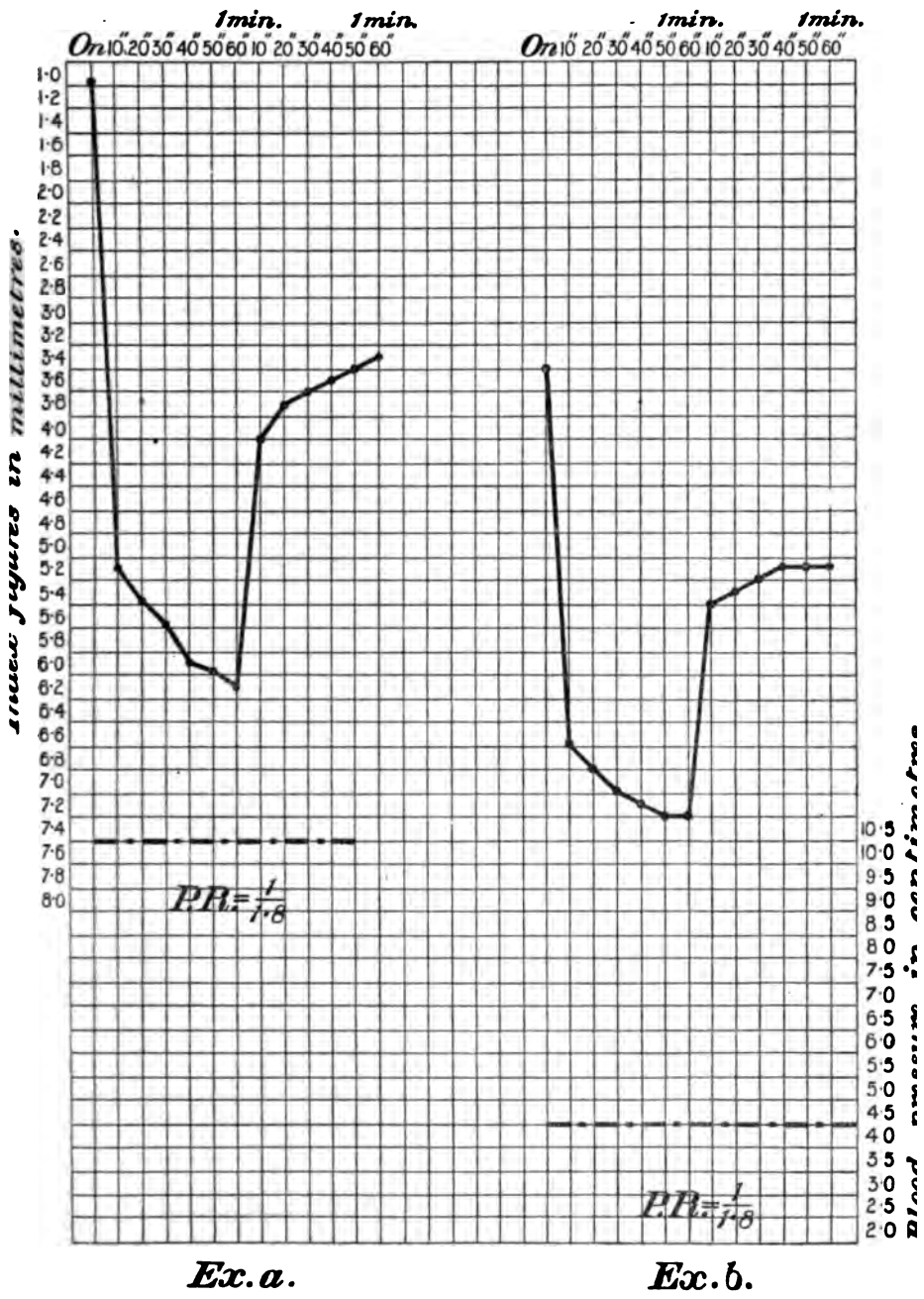
Observations were also made with a very short time of compression, such as of two seconds' duration. Under these circumstances the absolute excursion is notably less, viz., 2.4 to 3.5 mm. I have already mentioned the rapidity of the development of the first part of such a curve. The actual curve is shown in fig. 5.

ii. *Recoil*.—The character of the recoil after one minute compression with 50 grams is well shown in fig. 3. As a rule it is nearly completed in a few seconds, but develops further during one to two minutes, and then usually stops. After this point the process undoubtedly continues in many cases, but it is too gradual to record. After half to three-quarters of an hour, the brain in most cases will be found to have resumed its normal level. The recoil was measured at the point at which it became apparently stationary.

For convenience of expression, to denote the relation of the recoil to the compression, I employ a fraction, the numerator of which is the numerical value of the recoil expressed as unity, and the denominator is the value of the excursion compared to the recoil. Thus, where the excursion is twice the recoil the fraction =  $1/2$  ; and to the ratio I apply the term "proportionate recoil." The *smaller* the fraction the *less* the elasticity ; the nearer it approaches unity the more perfect the elasticity.

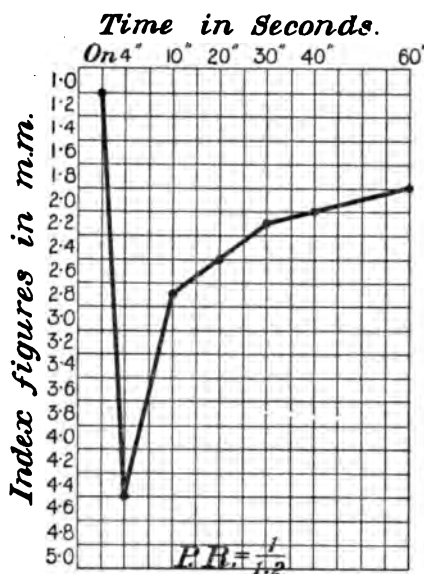
We are now in a position to see the degree to which the brain recovers itself after a compression of 50 grams for one minute. The proportionate recoil is very constant, i.e., nearly  $1/2$ , in many cases almost exactly so, but varying within the extreme limits of  $1/1.8$  to  $1/2.3$  (e.g., fig. 3 =  $1/1.9$ , fig. 4 =  $1/1.8$ ).

FIG. 4.



If, however, the pressure be kept up for only two to five seconds the recoil is more perfect. In fact, the immediate recoil which takes place in the first two or three seconds may give a ratio of  $1/2$  or even  $1/1.5$ ; after an interval of thirty seconds or so it may be  $1/1.4$  or even nearly 1. *E.g.*, Experiment 12a =  $1/1.2$  (fig. 5), Experiment 14a =  $1/1.5$  (immediate).

FIG. 5.



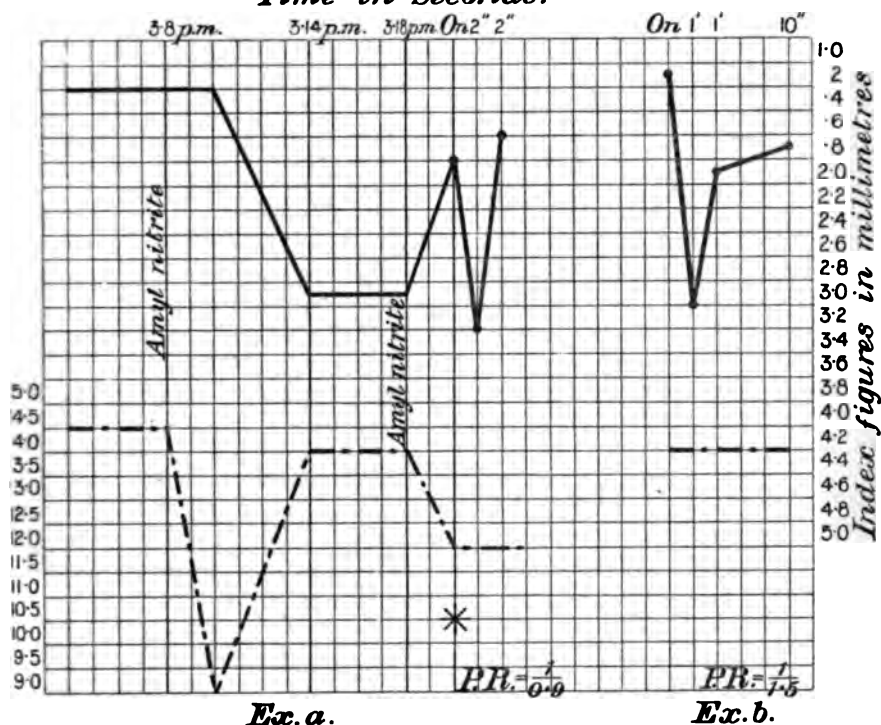
If the pressure be prolonged, say, for six minutes, the fraction becomes smaller, and this is shown in fig. 6, where the ratio =  $1/2.74$ , and the surface does not further recover after a long interval—more than half an hour.

Thus we see that although the elastic reaction of the brain is very fair when the compression lasts only a few seconds, longer compression affects the brain in such a fashion that it does not recover so readily.

Thus, also, we see that for the same weight and same length of compression, the elasticity is very much the same in different brains; *i.e.*, with a large excursion there is a proportionately large recoil, and *vice versa*.

iii. *Results of Experiments with varying Weights.*—A few experiments performed with a weight of 20 grams show a comparatively small excursion, but the ratio is similar to that obtained with a 50 grams weight. Weights such as 100 grams and over seem to be

FIG. 6.

*Time in Seconds.*

somewhat prejudicial to a good reaction; they produce a somewhat larger excursion. [A weight of 200 grams will gradually lacerate a dead brain.]

### C. Consideration of the Relation of the foregoing results to the Blood Pressure.

We may first take in this connexion the experiments in the series just described, and compare the reaction to the height of the blood pressure, to test the influence of the latter as a possible factor in the establishment of the recoil of the brain.

(a.) *Comparison of Experiments on the uninjured Brain in different Dogs with individually different, but normal, Blood Pressures.*—Below is a table of experiments performed on the normal brain in the course of which the blood pressure was measured.

It will be seen that there is absolutely no relation to be found between the individual "elasticities" and blood pressures.

| Experiment. |      | Time of<br>Compression. |      | Proportionate<br>recoil. |      | B. P.    |
|-------------|------|-------------------------|------|--------------------------|------|----------|
| 4a          | .... | 1 minute                | .... | 1/1·8                    | .... | 125 mm.  |
| 6a          | .... | 1    „                  | .... | 1/1·8                    | .... | 105    „ |
| 7a          | .. . | 30 seconds              | .... | 1/1·9                    | .... | 110    „ |
| 8a          | .... | 30    „                 | .. . | 1/2·2                    | .... | 150    „ |
| 9a          | .... | 30    „                 | .... | 1/2                      | .... | 105    „ |
| 12a         | .... | 4    „                  | .... | 1/1·2                    | .... | 140    „ |
| 14a         | .... | 2    „                  | .... | 1/1·14                   | .... | 125    „ |
| 20a         | .... | 2    „                  | .... | 1/2                      | .... | 120    „ |

The most noticeable of these is Experiment 8a, in which, although the pressure was comparatively very high, the reaction was almost at its minimum.

It is clear, then, that the recovery of the volume of the brain is not due to the pressure at which it is irrigated with blood.

Though these facts appeared to be thoroughly demonstrative, I established a number of control observations to further test the question.

The first method which suggested itself was naturally that of lowering the blood pressure very markedly by bleeding. This procedure, however, of necessity involves the measurement of the recoil of the brain thus rendered anæmic being taken from a preparation which had already been compressed once; this, of course, introduces an error, and hence the further question has to be considered how far does one compression experiment have an influence on a consecutive one.

When compression has lasted for thirty seconds or more it is obviously necessary to allow a sufficient interval to elapse to allow the damaged brain fairly to recover—such an interval as half to three-quarters of an hour, sometimes more; that is to say, I waited till the surface had regained its normal level. Sometimes it is impossible to wait for complete recovery, and, of course, in such a case an element of error is introduced; but when the time of compression is short, ten minutes is quite enough to allow so as to avoid any appreciable vitiation, of which fact I have satisfied myself experimentally.

(b.) *Comparison of Experiments performed on the same Brain before and after lowering the Blood Pressure by bleeding.*—After an experiment, conducted as above described at normal blood pressure, I bled the animal from the carotid artery.

As a rule, a large loss may occur without much fall of pressure, and the fall, when it does occur, takes place very suddenly. If the splanchnics be previously divided, the fall takes place earlier and more regularly. Usually I bled to the extent of 150 to 300 c.c.

By this means there could be produced an enormous fall of blood pressure, sometimes as much as 100 mm. (e.g., from a normal pres-

sure of 125 mm. down to one of 20 mm.). But, under these circumstances, the fall is due to a draining of blood from all parts of the body, the brain included, and hence the latter begins to collapse soon after the bleeding has been started. This collapse, however, is not always great. Thus, in one experiment (fig. 4), after allowing the brain to recover from the trial experiment, and then bleeding 150 c.c., the fall of blood pressure of 60 mm. thus produced only caused the brain to collapse so that its vertical thickness was reduced by not more than 2.4 mm.

Of course, this collapse produces a new set of intracranial conditions, *e.g.*, the brain contains less fluid, and hence its *texture* is altered; it does not fit its surroundings so closely—its vertical depth is less. These new conditions act in diverse ways as regards its elasticity.

But we cannot disregard the fact that the fall of blood pressure measured in the femoral artery is very great, and, were there any measurable direct relation between the elastic reaction and the blood pressure, this fall, especially when the brain collapse was not great, would make itself evident in spite of the altered conditions under which the second experiment has to be performed.

The following table gives the results of experiments of this kind (p. 14).

*Summary.*—In Nos. 4 and 8 there is a distinct diminution in recoil. In No. 6 the recoil is exactly the same in both experiments, and these being conducted under comparatively favourable circumstances as regards the fall of brain surface, this result is of more value than the others.

In No. 7 there is an actual increase in “elasticity.”

In every case the excursion is diminished after bleeding.

It will thus be seen that there is no constant relation between the fall of blood pressure and the proportionate recoil, and that in those cases in which a lowered pressure is accompanied by a lowered elasticity, the diminution is very small, and in no way commensurate with the fall. A series of bleeding experiments, performed without registering the exact fall of blood pressure, gave results similarly divergent.

(c.) *Comparison of the Results of Experiments in which the Blood Pressure was Varied by means of Amyl Nitrite.*—It was now evident that, if possible, a method should be found of reducing or raising the central arterial pressure without causing an alteration in the level of the brain surface. In order to do this, the blood supply to the brain must be kept up or diminished according as the central blood pressure falls or rises, *i.e.*, when the pressure falls the cerebral vessels should dilate, and *vice versa*.

Now, supposing a fall could be produced by a general vaso-motor action, brain arteries being included, here we should have the condi-

| No. of experiment. | Normal Brain. |                       |            | After Bleeding. |                       |            | Remarks.                                                                                                                     |
|--------------------|---------------|-----------------------|------------|-----------------|-----------------------|------------|------------------------------------------------------------------------------------------------------------------------------|
|                    | B. P. in mm.  | Proportionate recoil. | Excursion. | B. P. in mm.    | Proportionate recoil. | Excursion. |                                                                                                                              |
| 4                  | 120           | 1/1·8                 | 5·8        | 25              | 1/2                   | 3·9        | 50 grams applied for 1 minute.<br>Bleeding to the extent of 270 c.c.<br>No record of extent to which the brain surface sank. |
| 6<br>(Fig. 4)      | 105           | 1/1·8                 | 5·1        | 45              | 1/1·8                 | 3·8        | 50 grams applied for 1 minute.<br>Bled 150 c.c.<br>Fall of brain surface = 2·4 mm.                                           |
| 7                  | 120           | 1/2·3                 | 5·0        | 80              | 1/2·1                 | 2·7        | 50 grams applied for 30 seconds.<br>Bled 170 c.c.<br>Fall of brain surface = 3·8 mm.                                         |
| 8                  | 150           | 1/2·2                 | 5·7        | 62              | 1/2·47                | 3·7        | 50 grams applied for 30 seconds.<br>Bled 200 c.c.<br>Fall of brain surface = 3·5 mm.                                         |

tion required. The presence of vaso-motor fibres to the cerebral arteries has been denied, or, at any rate, they have never been demonstrated; but Roy and Sherrington\* have shown that the calibre of the cerebral vessels may be altered by certain substances, notably acids and alkalies, and they consider this a direct action on the vessel walls. It occurred to me to try the effect of amyl nitrite, and I found that I could obtain a good fall of blood pressure, whilst the brain surface would remain at the normal level, or, if previously depressed, it would rise.

It is an interesting fact, and one which has a bearing on the vaso-motor question just referred to, that the subsequent recovery of blood pressure is accompanied by a *fall* of the brain surface. This is well shown in fig. 6.

It is obvious from my experiments that the brain is, under the effect of the nitrite, undoubtedly flushed with blood to an extent sufficient to neutralise its tendency to collapse; whether by vaso-motor or local action on the vessel walls it matters little, as far as the purposes of my experiment go. I see no reason to believe that the flushing is a result of increased venous pressure, this being the alternative explanation usually suggested. I administered the drug by means of clinical capsules, one or two at a time, crushed, and placed under the ether funnel. I thus obtained differences in the general blood pressure of from 20 to 30 mm. of mercury, and yet the brain surface remained at a constant, or nearly constant, level.

The chief difficulty in this method lay in the short time during which the fall lasted, thus necessitating very prompt action so as to apply the compression at the right moment.

The results of four experiments are tabulated below (p. 16).

It is seen that in each case we find that with the lowered blood pressure the elasticity is increased, markedly so in Nos. 17, 18, and 20, thus still further emphasising the fact that the elasticity does not, or at any rate *need* not, depend on central blood pressure. It is also seen in the experiments performed under the influence of the nitrite that the excursion is less. Thus, then, we find a true increase of recoil under the vascular conditions induced by this drug.

*D. Comparison of the Results of Experiments in which the Effect of Increased Venous Pressure on the Recoil of the Brain was Measured.*

(a.) *Anatomical Considerations.*—These experiments were undertaken in view of the marked influence which variations in venous pressure are held to exert on cerebral vascular conditions.

\* Roy and Sherrington, "Regulation of the Blood Supply to the Brain," 'Journal of Physiology,' vol. 11.

| No. of experiment. | Normal Brain. |                       |            | During administration of Amyl Nitrite. |                       |            | Remarks.                                                                                                                                                                                               |
|--------------------|---------------|-----------------------|------------|----------------------------------------|-----------------------|------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
|                    | B. P. in mm.  | Proportionate recoil. | Excursion. | B. P. in mm.                           | Proportionate recoil. | Excursion. |                                                                                                                                                                                                        |
| 17<br>(Fig. 6)     | 140           | 1/1.5                 | 1.9        | 120                                    | 1/0.9                 | 1.3        | 20 grams applied for 2 seconds.<br>The small excursion as a result of a light weight is noticeable.<br>The ratio under amyl nitrite shows that the recoil is greater than the excursion. (See fig. 6.) |
| 18                 | 95            | 1/1.3                 | 3.3        | 70                                     | 1/1                   | 2.0        | 50 grams applied for 2 seconds.                                                                                                                                                                        |
| 19                 | 105           | 1/1.6                 | 3.2        | 90                                     | 1/1.5                 | 3.1        | 50 grams for 2 seconds.                                                                                                                                                                                |
| 20<br>(Fig. 7)     | 120           | 1/1.7                 | 3.4        | 90                                     | 1/1.23                | 3.2        | 50 grams for 1 second.                                                                                                                                                                                 |

To render this part of the paper more comprehensible, it is necessary to give a short description of the vessels providing for the outflow of blood from the dog's skull, as their anatomy presents many points of difference from that of the human being. The details are chiefly as stated by Ellenberger and Baum.\*

The greater part of the venous blood leaves the skull through the foramen magnum by route of the inferior occipital sinuses, and enters the venous plexus of the vertebral column.

The vein next in individual importance is the *superior cerebral*, which finds exit from a foramen situated, on either side, between the post-glenoid protuberance and the external auditory meatus. It communicates, through the medium of the *meatus temporalis*, with the lateral sinus of its own side. It is often of considerable size, and is one of the chief sources of the internal maxillary vein.

The internal jugular receives, along with other branches, the *inferior cerebral* vein, which runs in the carotid canal and opens into the cavernous sinus.

The internal jugular is very small, and carries away but little cerebral blood.

Running through the orbital fissures are important veins (ophthalmo-cerebral and cerebro-facial), which connect the cavernous sinuses with the external maxillary veins.

In addition to these, there are numerous emissary veins, and between all the channels of exit named there is free anastomosis.

(b.) *Method of Experiment.*—I exposed the accessible cranial veins, viz., the external and internal jugulars, and internal maxillaries; the latter I followed nearly to their termination in the superior cerebrals. I then blocked the various veins in succession by clamps, and performed the usual experiment upon the congested brain.

To make the experiments more complete in one case, I measured the change in the venous pressure in the lateral sinus by means of a cannula inserted into one superior cerebral vein and connected with a water manometer. Directions for dissecting out this vein are given by Gärtner and Wagner,† but I always found it necessary to remove the angle of the lower jaw in addition.

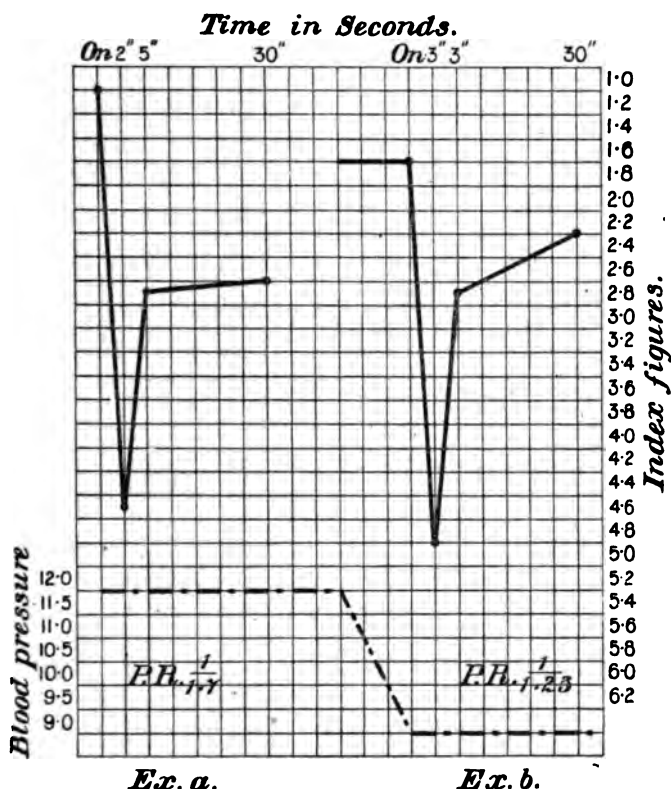
In the following table I have noted the degree of elevation of the brain surface, denoting the amount of congestion following blocking of the main venous outlets.

\* 'Anatomie des Hundes.'

† Wagner and Gärtner, 'Med. Wochenschrift,' Wien, 1887, Nos. 19 and 20.  
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| No. of experiment. | Normal Brain.         |            | Congested Brain.                    |                       |            | Remarks.                                                                                                                                                                                                                                                                                               |
|--------------------|-----------------------|------------|-------------------------------------|-----------------------|------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
|                    | Proportionate recoil. | Excursion. | Veins clamped.                      | Proportionate recoil. | Excursion. |                                                                                                                                                                                                                                                                                                        |
| 23<br>(Fig. 8)     | 1/1.6                 | 2.7        | Both superior cerebrals             | 1/1.25                | 2.5        | This was an experiment in which the change in the venous pressure was directly observed; the manometer registered 6.5 cm. in one superior cerebral, and this rose to 13 cm. when the other was clamped. The increase in the immediate recoil is also noticeable. (See fig. 8.) 50 grams for 2 seconds. |
| 25(a)              | 1/1.4                 | 4.5        | One superior cerebral               | 1/1.4                 | 4.4        | 50 grams applied for 5 seconds.                                                                                                                                                                                                                                                                        |
| 25(b)              | 1/1.4                 | 4.5        | Both superior cerebrals             | 1/1.48                | 3.7        | 50 grams applied for 5 seconds.                                                                                                                                                                                                                                                                        |
| 26(a)              | 1/1.36                | 3.8        | Both superior cerebrals             | 1/1.3                 | 3.2        | 50 grams applied for 5 seconds.                                                                                                                                                                                                                                                                        |
| 24(a)              | 1/1.6                 | 2.9        | Both internal jugulars              | 1/1.6                 | 2.8        | 50 grams applied 5 seconds: clamping these veins does not produce any perceptible expansion of the brain.                                                                                                                                                                                              |
| 24(b)<br>(Fig. 9)  | 1/1.6                 | 2.9        | Both external and internal jugulars | 1/1.4                 | 3.8        | 50 grams applied for 5 seconds. Surface (being previously depressed) rose 2 mm. on clamping the veins. The recoil is measured in this case at the end of 15 seconds only, the very marked recovery of the congested brain at the end of 2 minutes being shown in fig. 9.                               |
| 25(c)              | 1/1.4                 | 4.5        | Both external jugulars              | 1/1.4                 | 4.1        | 50 grams for 5 seconds. Brain surface rises 1.8 mm.                                                                                                                                                                                                                                                    |
| 26(b)              | 1/1.36                | 3.8        | Both external jugulars              | 1/1.4                 | 3.9        | Surface rises 1.6 mm. 50 grams applied for 5 seconds.                                                                                                                                                                                                                                                  |

FIG. 7.



Thus we see that, out of three experiments (24b, 25c, and 26b) in which both external jugulars were blocked, in only one (viz., 24b) is there an appreciable increase in recoil. (In this case, to be sure, the internal jugulars were also included, but, in the light of experiment 24a, I cannot believe that these insignificant veins are a factor worth considering.) Experiments 25a, 25b, and 26a show no increase of elasticity on blocking one or both superior cerebrals in any one of the cases, and hence are in strong contrast to 23a, in which the increased recoil is quite noticeable. This last experiment is also of importance, from the fact that the rise in venous pressure accompanying the increase in elasticity was actually measured, and it shows what a very considerable rise in the pressure (double the normal in this case) is necessary to influence elasticity at all, and is an indication of how far we may disregard the effect of small changes of venous pressure as factors vitiating results set forth in former sections of this paper.

FIG. 8.

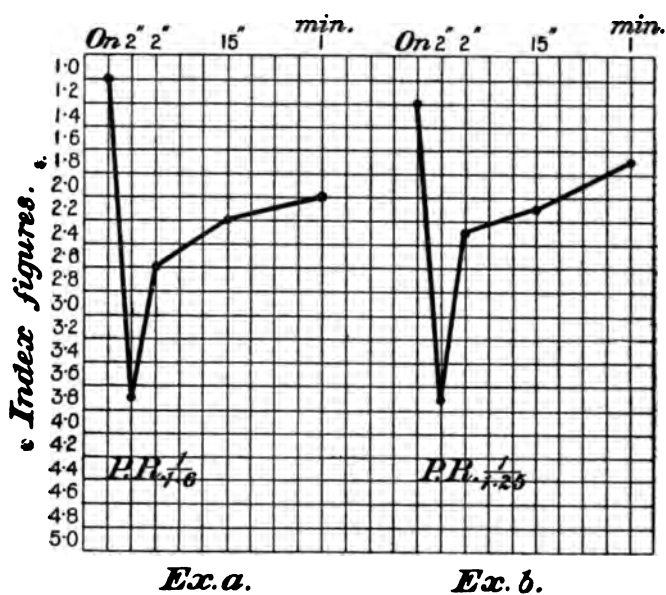
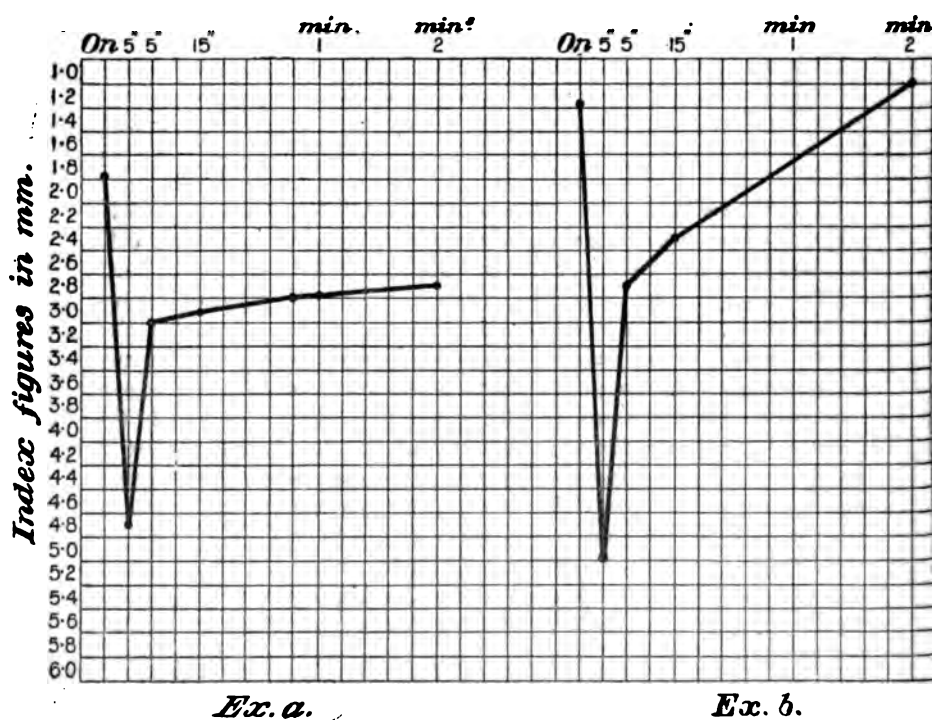


FIG. 9.



Experiments 25c and 26b show that a venous congestion of brain (as indicated by its expansion) is not necessarily accompanied by an increase of elasticity.

The failure to obtain changes in elasticity in every case must, I think, be attributed to the variation in calibre of the various channels of outflow, chiefly, probably, as regards the communications with the vertebral plexus; the common occurrence of variations in the size of the superior cerebral is patent after a few dissections.

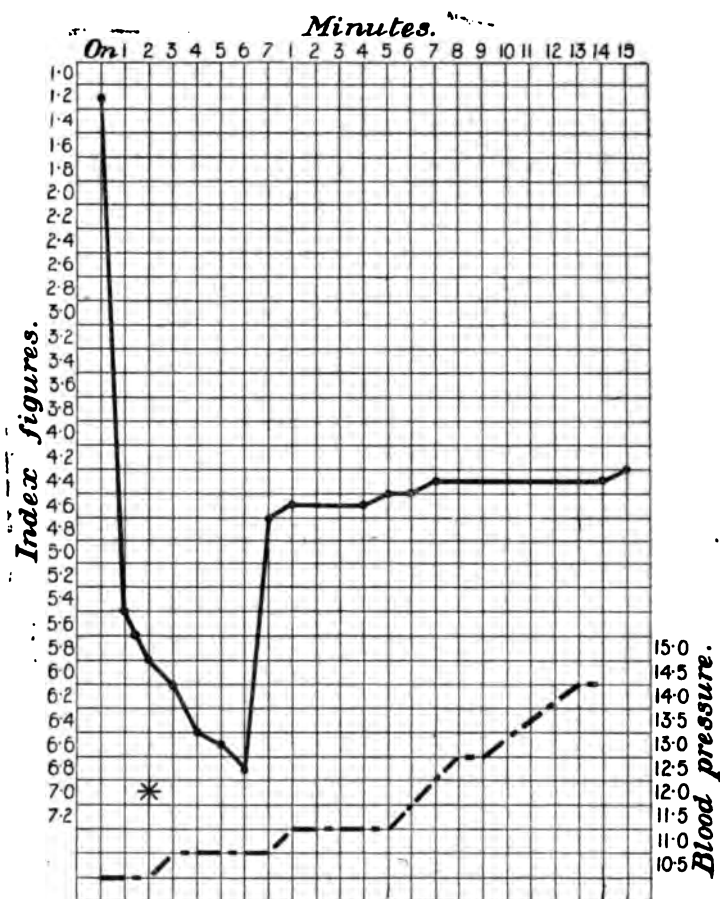
*E. Experiments illustrating the Influence which varying Vascular Conditions have in Restoring the Volume of a Brain which has undergone Prolonged or Severe Compression.*

(a.) *Influence of a Rise of the General Blood Pressure.*—After the brain has been compressed as described for six minutes, the elasticity, as before mentioned, is considerably reduced, and the brain surface continues depressed for a long period. If now the etherisation be diminished, the blood pressure may be caused to rise gradually, but considerably, without anything more than a comparatively insignificant rise in the level of the brain surface accompanying it (see fig. 10, in which the blood pressure rose 35 mm. in the space of 12 minutes). Thus we find a notable difference in the reactions of normal and compressed brains to increase of blood pressure. As is well known, in the uninjured brain the surface rises and falls concurrently with variations in the blood pressure.

(b.) *The Influence of Venous Congestion produced by Asphyxia.*—In cases of prolonged or severe compression the brain surface may nearly always be made rapidly to recover by inducing asphyxia. In an experiment (fig. 11) the brain had been severely compressed by repeated experiments on the same spot, so that the recoil was small and had become stationary in one minute. A rise of blood pressure of 30 mm., induced by diminishing the degree of etherisation, had no appreciable effect, but total recovery rapidly supervened with the development of asphyxia. This recovery cannot be ascribed to the rise in the general blood pressure induced by the asphyxia (in this case only 10 mm., which contrasts with the 30 mm. produced by diminishing the ether supply). This is also borne out by the fact that the recovery of the surface may be obtained when there is *no* rise of the blood pressure, as sometimes occurs during asphyxia, or the expansion may actually progress whilst the blood pressure is in its stage of fall.

Frequently the surface commences to recover when the asphyxial gasps are at their strongest, but recovery will go on when the respirations are small or even when they cease, as they will sometimes, directly the trachea is clamped.

FIG. 10.

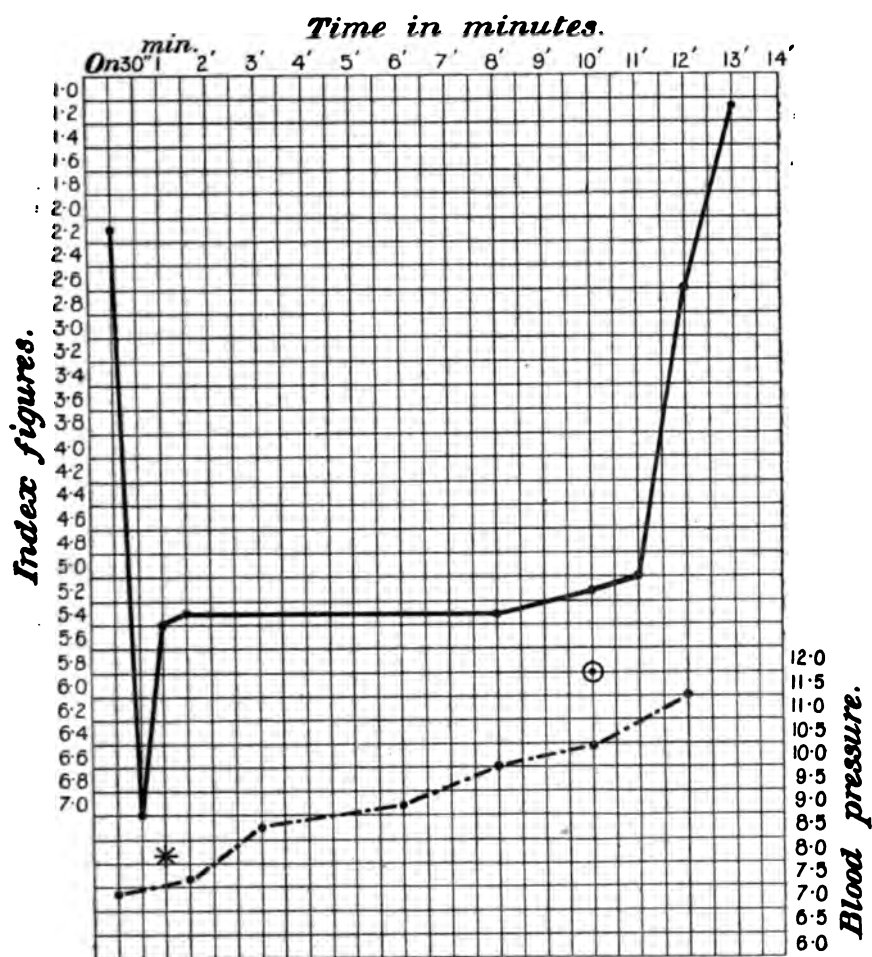


The extent of recovery varies in different cases; rarely it fails to take place at all.

It is important to notice that not only does the general surface of the hemisphere rise, but that the saucer-shaped depression due to compression is obliterated.

To obtain a definite idea of the extent to which the pressure in the cerebral veins rises during asphyxia, I took the venous pressure during asphyxia from the superior cerebral in two dogs. In one case in which the normal pressure was very low, about 1.5 of water, it quickly rose to 8 cm., and in the other in which the normal was 6.5 cm. water, after the usual preliminary fall, it rose again to 7.5 cm. The asphyxial gasps when strong may cause a variation in pressure amounting to 1 cm. water.

FIG. 11.



(c.) *Venous Pressure Varied by Artificial Respiration.*—By this means the surface may often be made to recover completely, or nearly so.

*Conclusion.*

The foregoing experiments show clearly that the elastic recoil of the living brain is due primarily to the elasticity of the brain substance, and is not due to the pressure in the arterial system (general).

The recoil, however, varies notably with changes in the pressure in the venous system (cerebral).

## DESCRIPTION OF DIAGRAMS.

The continuous lines represent the curves of the variations in level of the brain surface produced by pressure and otherwise. The dotted lines represent the height of the blood pressure.

The times in which compression and recoil are measured are noted above the diagrams in seconds. The enumeration of seconds always commences afresh at the point at which the weight is removed.

Fig. 3. 50 grams applied for 1 minute. This diagram shows typically the characters of the excursion and recoil.

Fig. 4. 50 grams applied for 1 minute. Between experiments *a* and *b* the brain was allowed to entirely recover its volume, and the animal was then bled to the extent of 150 c.c. This procedure caused a fall of the brain surface to 3·4.

Fig. 5. Shows a typical curve when 50 grams are applied for 4 seconds only.

Fig. 6. The curves as far as \* show the concurrent variations of brain surface and blood pressure under the influence of amyl nitrite. At \* experiment *a* was performed. Between experiments *a* and *b* the effect of the amyl nitrite was allowed to wear off completely. 20 grams was the weight employed.

Fig. 7. (*a*) was an experiment upon the normal brain.

(*b*) was performed after administration of amyl nitrite. Weight = 50 grams.

Fig. 8. (*a*) performed with a cannula in one superior cerebral vein.

(*b*) performed with the opposite superior cerebral also blocked. Weight = 50 grams.

Fig. 9. (*a*) performed on the normal brain.

(*b*) performed with both external and both internal jugulars clamped. Weight = 50 grams.

Fig. 10. 50 grams applied for 6 minutes. Etherisation diminished at \* with a consequent rise of blood pressure.

Fig. 11. 50 grams applied for 30 seconds to a brain which had undergone considerable previous compression. Etherisation diminished at \*. Trachea clamped at ○.

“On the Temperature of the Carbons of the Electric Arc; with a Note on the Temperature of the Sun. Experiments made at Daramona, Streete, Co. Westmeath.” By W. E. WILSON, M.R.I.A., and P. L. GRAY, B.Sc., A.R.C.S. Communicated by G. JOHNSTONE STONEY, F.R.S. Received November 14,—Read November 22, 1894.

The temperature of the positive pole of the electric arc, which is now generally believed to be the boiling point of carbon, is usually taken, on the authority of Violle,\* as approximately equal to 3500° C. Violle's method of determining it was as follows:—The carbons of the arc were placed horizontally, and the positive pole was so arranged

\* Violle, ‘*Jour. de Phys.*,’ 3rd Series, vol. 2, 1893, p. 545.

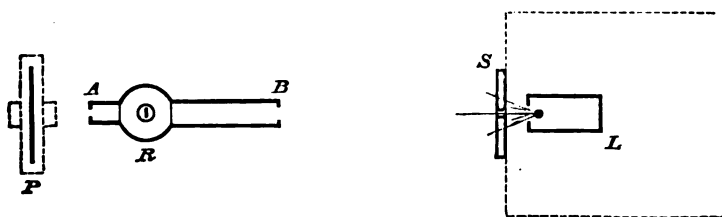
that pieces of its substance could be detached while the arc was passing; these white-hot pieces fell into a calorimeter, and from the amount of heat given up, the temperature was calculated, assuming the specific heat of carbon at this point to have its theoretical value. The method does not seem at first sight a very reliable one, and Violle states that the result is only to be regarded as an approximation.

The method adopted by the authors of this paper is exactly the same as that which they employed last year in their "Experimental Investigations on the Effective Temperature of the Sun,"\* in the account of which full descriptions of the apparatus used, &c., are given.

A Brockie-Pell arc lamp was employed in the experiments, the current being obtained from a dynamo worked by a gas engine. It would have been preferable for some reasons to have worked the arc off the 26 Epstein accumulators which we had at our disposal, but the current from these was used in heating the platinum strip, and we did not wish to run the cells off too quickly. Platinoid resistances were inserted in circuit with the arc until it burnt steadily.

The general arrangement of the apparatus is shown diagrammatically in Fig. 1.

FIG. 1.



P is the platinum-strip radiator (our modification of Joly's meldonometer), the dotted line representing the water-jacket which is placed over the strip. R is the radio-micrometer; A and B are tubes through which radiation can pass to fall on the receiving surfaces within. The diameter of the aperture at A is accurately known; as also is its distance from the receiving-surface, so that the apparent area of platinum, as seen from the latter, may be calculated.

L is the lamp, which is placed inside a wooden box, lined internally with tin-plate, both wood and metal being pierced with small holes opposite to the arc.

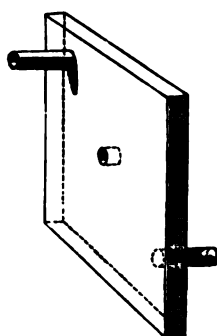
A screen, S, hangs in front of the box, and contains a small, carefully-measured hole, which can be adjusted until the brightest (or any) part of the glowing carbons shines directly into the tube B.

\* Wilson and Gray, 'Phil. Trans.,' A, vol. 185, 1894, p. 361.

The size of the hole in the screen S, and its distance from the radio-micrometer, then give the apparent area of bright carbon as seen from the latter.

The screen S is made of copper, and is really a flat box (Fig. 2)

FIG. 2.



provided with an inlet and an outlet tube, so that a continual stream of water from the ordinary house supply could be kept running through it—a precaution necessary from its proximity to the arc. In the experiments, a plentiful stream was kept running through this box, and thence on to the water-jacket round the radiator, the supply being sufficient to prevent any perceptible heating of the screen.

A small hole cut in the side of the wooden box enabled us, with the aid of mirrors, to use a pencil of the light of the arc for reflection from the mirror of the radio-micrometer, thus obviating the necessity of a lime-light, or other bright source, while an incandescent lamp-filament provided us with an extremely sharp band of light on the scale of the radiator. (A larger and better mirror had been affixed to this since its use in our work on the solar temperature, and this mirror, with an incandescent lamp, gives a band of light with edges so sharp on the "temperature scale," that it could, if necessary, be read to the tenth of a millimetre, which is beyond our ordinary requirements.)

The theory of the method is very simple; essentially it is the same as that which applies to the estimation of the effective temperature of the sun, without the complications arising from atmospheric absorption, &c.

In the case of the sun, we can only hope to find (at least at present) the *effective* temperature, as we know little of the radiating power of the photospheric substances, but in the case of the carbons of the arc,

we may assume that we are dealing with a "black" surface of approximately unit emissive power.

Let then

$T_1$  = absolute temperature of bare platinum-strip at balancing point,

$e$  = ratio of emissive power of a black surface to that of the bare metallic surface at this temperature,

$A$  = ratio of the area subtended by the platinum to that subtended by the glowing carbon, at the receiving surface of the radio-micrometer,

and  $q = f(T)$  be the "law of radiation" for a black surface, where  $q$  = quantity of radiation as a function of the absolute temperature  $T_1$  of the radiating surface.

Then the radiation from the carbon is  $A/e$  times the intensity of that from a black surface at a temperature  $T_1$ .

If the radiation from the platinum at the temperature  $T_1$  be put =  $q_p$ ,

then  $q_p = f(T_1)$ .

Then the radiation from the carbon

$$= \frac{A}{e} \cdot q_p = \frac{A}{e} \cdot f(T_1).$$

And if  $T_2$  = required temperature of the carbon

$$\frac{A}{e} \cdot f(T_1) = f(T_2),$$

whence  $T_2$  may be obtained, when we know (1) the law of radiation, (2) the ratio of the emissive powers of bare and blacked platinum.

We go on to discuss these two points together, as the experiments on the first give us information on the second.

#### *The Law of Radiation and the Ratio of the Emissive Powers.*

In our paper already quoted we have given a series of experiments on the radiation from bare platinum at temperatures up to 1600° C. approximately, and we have shown that a simple fourth-power law expresses the results very closely, so that for these experiments the "law of radiation" is  $q = a(T^4 - T_0^4)$ , where  $T$  = absolute temperature of radiating surface,  $T_0$  = temperature of surrounding medium,  $a$  = a constant, and  $q$  = radiation in arbitrary units. At high temperatures  $T_0^4$  becomes unimportant, and the expression simplifies still further to  $q = aT^4$ .

Experiments on a blackened surface are difficult to carry out at

anything beyond moderately low temperatures; we therefore assumed in our former work that the form of the law was the same for a blackened as for a bright surface, there being good grounds, both theoretical and experimental, for such a belief. Further investigations, however, indicate that this assumption is not correct, as will be seen from the experiments detailed below.

A series of experiments on the radiation from bare platinum was made first, exactly in the same way as those described in our work of last year, that is to say, the radiation from the platinum at different temperatures was allowed to fall on a radio-micrometer of the ordinary form, the sensibility of which was reduced sufficiently to give a readable deflection at the highest temperature used, the deflection as given by the scale-readings being then taken as proportional to radiation. This proportionality has been shown before to be strictly true for deflections up to and greater than those obtained in these experiments.

The platinum-strip was next blackened on one side with black oxide of copper, which was ground very fine, mixed up with methylated spirit, and laid on with a camel's-hair brush; this, when the liquid had dried off, gave a very good, even, dead-black surface, the emissive power of which may be taken as approximately equal to that of an ideal black surface.

Lampblack, of course, is useless for these experiments, since it burns off at something under  $500^{\circ}\text{C.}$ ; it could only be used if the radiator could be placed in a vacuum, or in an atmosphere having no action on the carbon, for which purpose we are having apparatus specially constructed.

At about  $900^{\circ}\text{C.}$  the black oxide of copper begins to suffer a change; its surface becomes somewhat shiny, and an alloy is formed with the platinum; this puts a limit to the temperature at which the radiation may be taken as that of a "black" surface. Our first strip was spoiled in discovering this limiting temperature; the second strip (after calibration, &c., and radiation experiments with the metal bare) was covered on both sides and examined during the progress of the experiments, which was stopped as soon as the black surface showed any signs of change of physical condition; these were not only apparent to the eye, but were also immediately indicated by a variability of temperature, due to the alteration of emissive power, as the reduction of the oxide crept over the surface of the strip.

Platinum-black would have no advantages in this connection over the copper-oxide, as it reverts to the metallic condition at very nearly the same temperature as that at which the oxide changes in the way mentioned above.

The two series of experiments gave the figures in the following tables:—

The 1st column gives the absolute temperature of the platinum.

„ 2nd „ „ deflection on the scale of the radio-micro-meter in millimetres. These numbers then represent radiation in arbitrary units.

„ 3rd „ „ radiation calculated by a formula to be discussed immediately.

„ 4th „ „ differences between the observed and calculated radiation.

Table I.—Radiation of Bare Platinum.

| Abs. temp. | Radiation observed. | Radiation calculated. | Obsd.—calc. |
|------------|---------------------|-----------------------|-------------|
| 637°       | 7·0                 | 10·2                  | −3·0        |
| 771        | 17·5                | 18·8                  | −1·3        |
| 804        | 22·5                | 22·5                  | −0·0        |
| 888        | 33·0                | 32·4                  | +0·6        |
| 896        | 36·5                | 35·5                  | +1·0        |
| 979        | 52·7                | 51·5                  | +1·2        |
| 1008       | 59·2                | 58·2                  | +1·0        |
| 1033       | 65·0                | 64·4                  | +0·6        |
| 1044       | 68·0                | 67·3                  | +0·7        |
| 1049       | 68·5                | 78·7                  | −0·2        |
| 1183       | 94·5                | 94·5                  | 0·0         |
| 1155       | 99·5                | 102·3                 | −2·8        |

Table II.—Radiation from Blacked Platinum.

| Abs. temp. | Radiation observed. | Radiation calculated. | Obsd.—calc. |
|------------|---------------------|-----------------------|-------------|
| 683°       | 67                  | 67                    | 0           |
| 798        | 115                 | 115                   | 0           |
| 893        | 166                 | 163                   | −2          |
| 933        | 193                 | 195                   | −2          |
| 967        | 218                 | 213                   | +5          |
| 975        | 235                 | 227                   | +8          |
| 1024       | 269                 | 268                   | +1          |
| 1058       | 304                 | 299                   | +5          |
| 1075       | 314                 | 316                   | −2          |
| 1107       | 349                 | 349                   | 0           |

The same results are shown graphically in fig. 3, in which the curve is drawn from the formula through points denoted thus ©, while the experimental points are denoted thus □.

The curve for bare platinum was taken first, and a simple fourth-power law tried on it; this was found to agree very closely with the observed results throughout the range of the experiments, except at

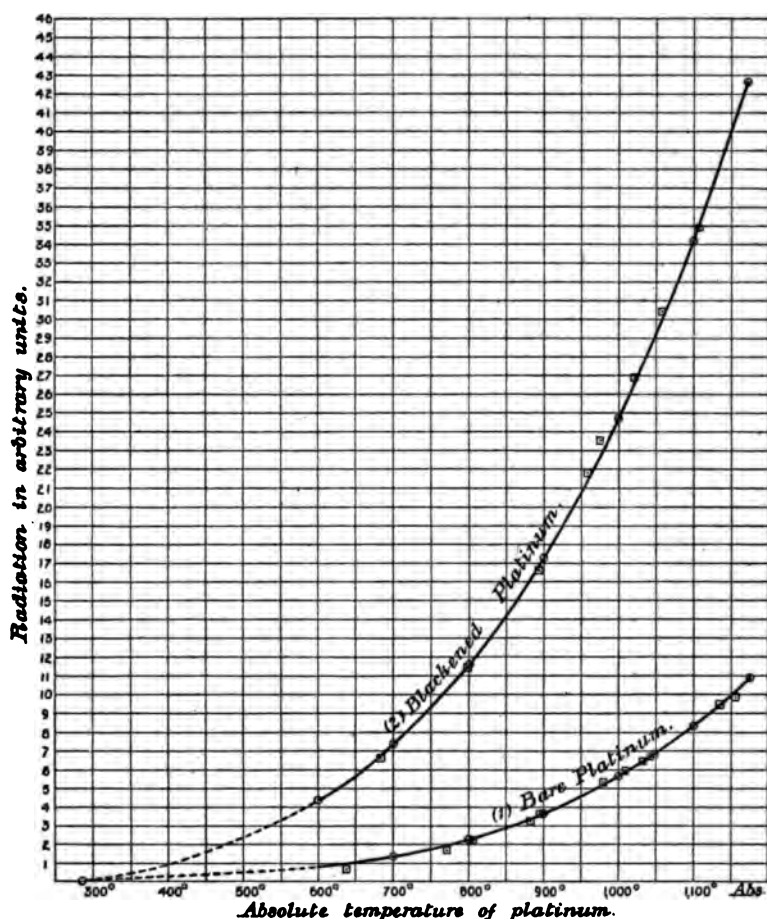


FIG. 3.—Curve showing the relation of radiation to temperature in the case of (1) bare platinum, (2) blacked platinum.

the lowest temperatures, thus confirming our work of last year, but in the case of the blacked platinum the curve is much less steep than that given by a fourth-power law. In fact, after several trials it was found that the exponent 3.4, in the expression  $q = aT^x$  followed the curve fairly well, but that a formula of the form

$$q = a(T^3 - T_0^3) + b(T^4 - T_0^4),$$

would fit both curves, the values of  $a$  and  $b$  being obtained from the respective experiments in the two cases.

In this expression, as before,  $q$  represents quantity of radiation,  $T$  the absolute temperature of the radiating surface,  $T_0$  the

temperature of its surroundings, while  $a$  and  $b$  are constants determined in any case from two experimental points at some distance apart. When these have been calculated, the expression may be simplified by writing  $aT_0^3 + bT_0^4 = a$  third constant,  $c$  say, so that we have

$$q = aT^3 + bT^4 - c,$$

$c$  being very small and unimportant when any but very low values of  $T$  are concerned.

The curves (fig. 3) and figures (Table I and Table II) already given were obtained from expressions of this form ; for bare platinum the constants were calculated from

$$\begin{aligned} T &= 804^\circ \text{ abs.}, q = 22.5 \\ \text{and } T &= 1133 \quad ,, \quad q = 94.5 \end{aligned}$$

from this, the values obtained are

$$\begin{aligned} \log a_1 &= \bar{9}.92855 \\ \log b_1 &= \bar{11}.81280 \end{aligned}$$

$a$  being, however,  $-$ , and  $b$   $+$ .

$T_0$  was always about 288 (i.e., the temperature of the room was  $15^\circ \text{ C.}$ ), and in this case,  $c$  comes out  $= 0.24$  (i.e., 0.24 mm. on the scale of the radio-micrometer), which is practically negligible.

For blacked platinum the constants were calculated from the experimental points

$$\begin{aligned} T &= 683^\circ \text{ abs.}, q = 67.0 \\ T &= 1107 \quad ,, \quad q = 349.0 \end{aligned}$$

whence we obtain

$$\begin{aligned} \log a_2 &= \bar{7}.22166 \\ \log b_2 &= \bar{11}.92915, \end{aligned}$$

both  $a$  and  $b$  being  $+$ , while  $c = 4.6$ , so that for calculating the radiation, in our arbitrary units, at any temperature, we have

$$\begin{aligned} q &= a_2T^3 + b_2T^4 - 4.6 \text{ for blacked platinum,} \\ \text{and } q &= a_1T^3 + b_1T^4 - 0.2 \text{ for bare platinum,} \end{aligned}$$

the constants being those given above for blacked and bare platinum respectively.

From the two radiation curves, for bare and blacked platinum respectively, we may obtain the relative values of the emissive powers at different temperatures. That the ratio is not constant has been known for some time;\* the table given below will show the nature and extent of the variation.

\* Schleiermacher, 'Wied. Ann.', vol. 26, 1885, p. 287. Also Wilson and Gray's paper already quoted, p. 380, in which several references will be found, relating to experiments on this point, and the law of radiation, &c.

The radiation is calculated from the formulæ given on p. 31, for temperatures 600°, 700° .... 1200° abs.; the fourth column gives the ratio black/bare.

Table III.—Ratio of Emissive Powers of Black and Bare Platinum.

| Abs. temp. | Radiation of black platinum. | Radiation of bare platinum. | Ratio. |
|------------|------------------------------|-----------------------------|--------|
| 600°       | 42·4                         | 6·4                         | 6·62   |
| 700        | 73·0                         | 12·5                        | 5·84   |
| 800        | 116·1                        | 22·0                        | 5·28   |
| 900        | 173·2                        | 36·2                        | 4·78   |
| 1000       | 247·0                        | 56·3                        | 4·39   |
| 1100       | 341·5                        | 83·6                        | 4·09   |
| 1200       | 459·4                        | 119·9                       | 3·83   |

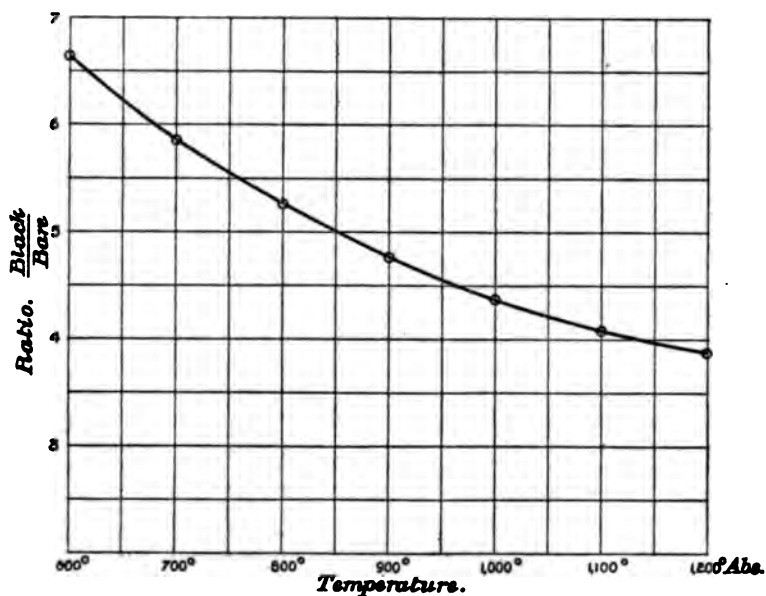


FIG. 4.—Curve showing the ratio of the emissive powers of blacked and bare platinum at different temperatures.

The curve (fig. 4) shows the same results graphically.

It will be seen at once that the ratio diminishes as the temperature increases, but less and less rapidly. The observations agree fairly well with the old experiments generally described in the text-books—experiments made at 100° C. and thereabouts—in which the ratio

is given as from 10 to 12. They differ slightly from Schleiermacher's series, the ratio in his being a little lower than in ours at the same temperature.

There is a fair agreement with Rossetti's result at high temperatures; he obtained 2.9 at about 1500° C., which is very little lower than the value which would be reached by a theoretical continuation of our curve (fig. 4).

The tendency of the curve appears to be to approach a constant value of about 3, but it is impossible to dogmatise on this point, from the physical limitations of the inquiry. The fact that the ratio *does* diminish shows that the physical nature of one or both of the surfaces is different at different temperatures; probably it is the bare platinum which changes, and it is possible that the constant value of the ratio is just attained at the melting-point of the metal. Experiments with a molten surface would be interesting, but very difficult to carry out.

#### *The Balancing Experiments.*

We may now pass on to the "balance experiments," from which the temperature of the carbons of the arc may be calculated. The principle of the method has already been described in the earlier part of the paper.

When the arc was shining on the top receiving-surface of the radio-micrometer, and the incandescent platinum on the lower, the position is denoted as Position A, the reverse, by Position B.

The angle subtended by the area of incandescent platinum, in both cases, was 5°301.

The diameter of the hole in the screen (fig. 2), through which the radiation of the arc passed, was 0.337 cm. In position B, its distance from the receiving surface was 57.0 cm.; in position A it was 58.2 cm., giving angular apertures of 0.339° and 0.332° respectively. The screen was close enough to the arc to make it certain that the hole was completely filled with the brightest part of the crater of the + pole. The hole was sufficiently small to form a rough "pin-hole" image of the carbons, by means of which it could be seen during an experiment that the brightest part of the + pole was shining directly into the tube of the radio-micrometer, and so on to the receiving surface.

In position B, the ratio of the areas of platinum and carbon, as seen from the receiving-surface, was

$$(5.301/0.339)^2 = 245,$$

and in position A, the ratio was

$$(5.301/0.332)^2 = 255.$$

The following are the temperatures at which the radiation from the bare platinum balanced that from the hottest part of the + pole:—

| Position B. 715° C.        | Position A. 862° C.         |
|----------------------------|-----------------------------|
| 717                        | 930                         |
| 737                        | 924                         |
| 720                        | 922                         |
| 732                        | 902                         |
| 722                        | 933                         |
| 722                        | 985                         |
|                            | 945                         |
| Mean = 724° C. = 997° abs. | Mean = 925° C. = 1198° abs. |

The results in position A are not so concordant as those in position B, but the arc was not quite so steady; the first low reading (862°) was probably taken when the growth of condensed carbon on the — pole was partly shading the receiving surface from the heat of the crater, while the high reading (935° C.) probably corresponds to one of those sudden “bursts” of high temperature which we have frequently observed to take place, although we cannot offer any explanation of them.

The current in the above experiments was about 14 ampères; a few observations were subsequently made with less resistance in circuit, and a current of about 25 ampères; the temperature then appeared to be a little higher than with the smaller current, but the arc in this case was so unsteady as to prevent the observations being made very carefully.

[Later experiments with a higher voltage (110 volts) and a current varying from 10 to 40 ampères indicate an exact equality of temperature, which confirms the usually-accepted view.—April 8th, 1895.]

Working out the results of the two positions separately, we have, for the ratio of emissive powers, at

$$997^{\circ} \text{ abs.}, \frac{\text{black}}{\text{bare}} = 4.4.$$

$$1198 \quad \text{,,} \quad \text{,,} \quad 3.85.$$

From the formula (p. 31), for blacked platinum,

$$q = aT^3 + bT^4 - 4.56,$$

we have for position B,  $q = 244.48$ ,

and for      „      A,  $q = 456.87$ .

Therefore we have a radiation from the +pole of the arc, in our arbitrary units, corresponding to

$$244.48 \times \frac{245}{4.4} = 13613 \text{ in position B,}$$

and to

$$456.87 \times \frac{255}{3.83} = 30420 \text{ in position A.}$$

The geometrical mean of these two numbers,

$$\sqrt{13613 \times 30420} = 20350,$$

is the true radiation in our arbitrary units. To find the corresponding temperature, we must substitute this value in the equation on p. 31, i.e.,

$$20350 = aT^3 + bT^4 - 5.$$

This is most easily solved by trial, T coming out as 3520°.

To this about 100° must be added, owing to the curvature of the radiation curve (for full reason, see Wilson and Gray's paper already quoted, p. 387), giving approximately

$$3600^\circ \text{ abs., or } 3300^\circ \text{ C.}$$

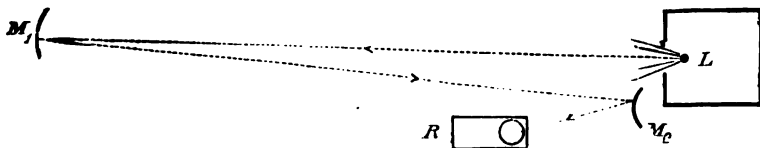
as the temperature of the hottest part of the +pole of the electric arc, a result surprisingly near Violle's estimate, 3500° C.

#### *The Comparative Radiation from Different Parts of the Arc.*

After the above experiments had been finished, an attempt was made to obtain comparative values of the radiation, and hence the temperatures, of different parts of the carbons of the arc. For this purpose, a radio-micrometer of the ordinary form was employed, on to the receiving-surface of which radiation could fall through a large pin-hole.

An image of the carbons was then formed by an arrangement shown diagrammatically in the figure (fig. 5), in which L is the arc

FIG. 5.



lamp, inside a lantern with the condenser removed,  $M_1$  is a concave mirror,  $M_2$  a convex mirror, both silver-on-glass, and R is the radio-

micrometer. The reason for not forming a direct image with a lens was the varying transparency of glass for radiation at different temperatures; the mirrors also enabled us to "dilute" the heat considerably, and so obtain convenient direct deflections on the radio-micrometer scale.

The sketch (fig. 6) shows approximately the shape of the image formed, on a scale about two-thirds full size.

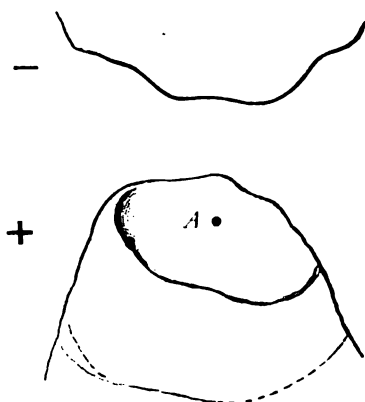


FIG. 6.—Image of the carbons from a tracing. A represents the size of the aperture by which radiation reached the receiving-surface.

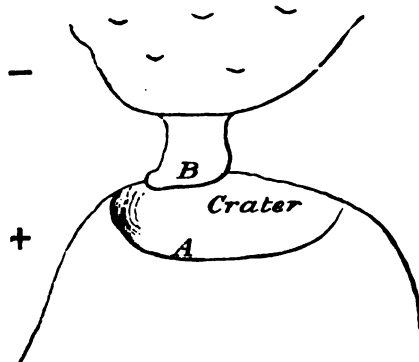
The mirror,  $M_2$ , being provided with adjusting screws, it was easy to bring any part of the image, either of the carbons or the pale violet glow of the arc itself, on to the small aperture, the deflection on the scale of the radio-micrometer then giving readings proportional to the radiation from the chosen point.

Magnified to this extent, however, the arc was never steady enough to allow a detailed "mapping-out" of the carbon surfaces with regard to temperature. Even when the light is apparently steady to the eye, the violet arc itself often shifts its position, while the  $-$  pole continually alters in shape from the carbon deposited on it, which causes a bulbous excrescence, somewhat as shown in fig. 7, to form gradually.

When this is the case, the arc naturally strikes across from some such position as A to B; B then becomes, as might be expected, much hotter than any other part of the  $-$  pole.

As an example of the kind of difference existing between the two poles, the following figures may be given; they correspond to the hottest obtainable point in the crater of the  $+$  pole, and to the hottest point on the  $-$  pole, before any excrescence has had time to grow.

FIG. 7.



The numbers are scale divisions on the scale of the radio-micrometer, and therefore represent the radiation in arbitrary units:—

+ pole. Radiation 60.2 and 67.1  
Mean = 63.7  
— pole. Radiation = 21.8,

so that the radiation from the hottest part of the + pole was about three times as great as that from the hottest part of the — pole.

Taking the temperature of the former as 3300° C., this would give a temperature of about 2350° C. for the latter.

In a case where a “blob” had formed on the — pole, as in fig. 7, the following readings were obtained:—

+ pole. Radiation 57.0, 56.0, 56.6, and 55.5  
Mean = 56.3  
— pole. Radiation of hottest part = 38.3.

Again, taking 3300° C. for the former, this gives about 2700° C. for the latter, or 350° C. higher than that of the — pole just after the arc is started.

We may say, then, that if the temperature of the crater is about 3300° C., that of the — carbon is ordinarily about 2400° C. in its hotter parts.

As for the temperature of the arc itself, we can say nothing. Allowing the pale violet glow between the poles to fall on the aperture of the radio-micrometer, we obtained deflections of from 1 to 2 per cent. of those obtained when the hottest part of the crater was used, which seems to indicate a comparatively high radiative power for the hot gases which lie between the carbon poles.

### 38 *On the Temperature of the Carbons of the Electric Arc, &c.*

#### *Note on the Effective Temperature of the Sun.*

In the authors' work on this subject, radiation experiments were made with bare platinum, at temperatures up to 1600° C. approximately, and it was assumed that a formula of the same form as expressed these results would also hold for a blacked surface, while the ratio of the emissive powers at high temperatures was taken on Rossetti's authority as about 2.9.

The new work, given above, appears to show that the curve for the black surface does not, however, follow a simple fourth-power law so closely as does that for the bare platinum, and that, taking the law as given on p. 31 of the present paper, a correction must be made to the result obtained by the earlier work.

The approximate value of this correction may be obtained by taking the figures given as a typical case on p. 386 of last year's paper, and applying the new law to them.

In this case, the corrected ratio (*i.e.*, the ratio corrected for atmospheric absorption, and for loss by reflection from the glass of the heliostat) of the apparent areas of the bare platinum and the sun was approximately 1295 : 1, and balance was obtained with the platinum at a temperature of 1514° Abs.

Now by the formula on p. 31, the radiation of bare platinum at this temperature

$$= a \cdot 1514^3 + b \cdot 1514^4 - 0.27 = 311.77$$

$a$  and  $b$  having the values given on p. 31.

The radiation from the sun therefore

$$= 1295 \times 311.77 = 403,450.$$

To find the effective temperature of the sun, we have, therefore, to solve the equation

$$403,450 + 4.6 = aT^3 + bT^4,$$

where  $a$  and  $b$  now have the values corresponding to the curve for the black surface. This gives  $T = 7800^\circ$  Abs., approximately, instead of  $7000^\circ$ , as given by the older method of working.

That is to say, supposing the new formula to be correct, our estimate of the solar temperature would have to be increased by something like  $800^\circ$ .

If, however, the ratio of the emissive powers approaches a constant value, as the figures and curves on p. 32 make possible, the expression for the curve of the black surface would be somewhat altered, in such a direction as to reduce the correction, so that we may say finally that, taking Ångström's estimate of the atmospheric absorption, which gave in our former work an effective solar temperature of  $7400^\circ$  C., its more probable value would now be not very far from  $8000^\circ$  C.

"The Stresses and Strains in Isotropic Elastic Solid Ellipsoids in Equilibrium under Bodily Forces derivable from a Potential of the Second Degree." By C. CHREE, M.A., Fellow of King's College, Cambridge, Superintendent of Kew Observatory. Communicated by Professor W. G. ADAMS, F.R.S. Received March 2,—Read May 10, 1894. Abridged February 20, 1895.

General Formulæ.

§ 1. Let the isotropic elastic solid ellipsoid,

$$a^{-2}x^2 + b^{-2}y^2 + c^{-2}z^2 = 1 \dots\dots\dots (1),$$

of uniform density  $\rho$ , be acted on by bodily forces whose components  $Px$ ,  $Qy$ ,  $Rz$  are derivable from a potential

$$V = \frac{1}{2} (Px^2 + Qy^2 + Rz^2) \dots\dots\dots (2).$$

Let  $\Pi$  denote the determinant

$$\begin{vmatrix} 3b^4 + 2b^2c^2 + 3c^4, & c^4 - \eta(b^2c^2 + c^2a^2 + 3a^2b^2), & b^4 - \eta(b^2c^2 + 3c^2a^2 + a^2b^2) \\ c^4 - \eta(b^2c^2 + c^2a^2 + 3a^2b^2), & 3c^4 + 2c^2a^2 + 3a^4, & a^4 - \eta(3b^2c^2 + c^2a^2 + a^2b^2) \\ b^4 - \eta(b^2c^2 + 3c^2a^2 + a^2b^2), & a^4 - \eta(3b^2c^2 + c^2a^2 + a^2b^2), & 3a^4 + 2a^2b^2 + 3b^4 \end{vmatrix} \dots\dots\dots (3)$$

and let its minors be  $\Pi_{11}$ ,  $\Pi_{12}$ , &c., where  $\Pi_{12} = \Pi_{21}$ .

Then with the notation of Todhunter and Pearson's "History,"  $\eta$  denoting Poisson's ratio, the stresses are given by

$$\left. \begin{aligned} \widehat{xx} &= a^2 \left[ \left( \frac{1}{2}P\rho + M + N \right) \left( 1 - \frac{x^2}{a^2} \right) - \left( \frac{1}{2}P\rho + M + 3N \right) \frac{y^2}{b^2} \right. \\ &\quad \left. - \left( \frac{1}{2}P\rho + 3M + N \right) \frac{z^2}{c^2} \right], \\ \widehat{yy} &= b^2 \left[ - \left( \frac{1}{2}Q\rho + 3N + L \right) \frac{x^2}{a^2} + \left( \frac{1}{2}Q\rho + N + L \right) \left( 1 - \frac{y^2}{b^2} \right) \right. \\ &\quad \left. - \left( \frac{1}{2}Q\rho + N + 3L \right) \frac{z^2}{c^2} \right], \\ \widehat{zz} &= c^2 \left[ - \frac{1}{2}(R\rho + L + 3M) \frac{x^2}{a^2} - \left( \frac{1}{2}R\rho + 3L + M \right) \frac{y^2}{b^2} \right. \\ &\quad \left. + \left( \frac{1}{2}R\rho + L + M \right) \left( 1 - \frac{z^2}{c^2} \right) \right], \\ \widehat{yz} &= 2Lyx, & \widehat{zx} &= 2Mzx, & \widehat{xy} &= 2Nxy \end{aligned} \right\} \dots\dots (4);$$

$$\text{where } L = \frac{\rho}{2\Pi} [Pa^2\{\eta(b^2+c^2)\Pi_{11}+(\eta c^2-a^2)\Pi_{12}+(\eta b^2-a^2)\Pi_{13}\} \\ + Qb^2\{(\eta c^2-b^2)\Pi_{11}+\eta(c^2+a^2)\Pi_{12}+(\eta a^2-b^2)\Pi_{13}\} \\ + Rc^2\{(\eta b^2-c^2)\Pi_{11}+(\eta a^2-c^2)\Pi_{12}+\eta(a^2+b^2)\Pi_{13}\}]. \dots (5),$$

while M and N are got from L by replacing the first suffix in the  $\Pi$ 's by 2 and by 3 respectively.

Denoting the displacements by  $\alpha, \beta, \gamma$ , types of the 6 strains are

$$\left. \begin{aligned} \epsilon_x &\equiv \frac{dx}{dx} = \frac{1}{E} \left[ \left( \frac{1}{2}P\rho + M + N \right) a^2 - \eta \left[ \left( \frac{1}{2}Q\rho + N + L \right) b^2 \right. \right. \\ &\quad \left. \left. + \left( \frac{1}{2}R\rho + L + M \right) c^2 \right] \right. \\ &\quad - \frac{x^2}{a^2} \left\{ \left( \frac{1}{2}P\rho + M + N \right) a^2 - \eta \left[ \left( \frac{1}{2}Q\rho + 3N + L \right) b^2 \right. \right. \\ &\quad \left. \left. + \left( \frac{1}{2}R\rho + L + 3M \right) c^2 \right] \right\} \\ &\quad - \frac{y^2}{b^2} \left\{ \left( \frac{1}{2}P\rho + M + 3N \right) a^2 - \eta \left[ \left( \frac{1}{2}Q\rho + N + L \right) b^2 \right. \right. \\ &\quad \left. \left. + \left( \frac{1}{2}R\rho + 3L + M \right) c^2 \right] \right\} \\ &\quad \left. - \frac{z^2}{c^2} \left\{ \left( \frac{1}{2}P\rho + 3M + N \right) a^2 - \eta \left[ \left( \frac{1}{2}Q\rho + N + 3L \right) b^2 \right. \right. \right. \\ &\quad \left. \left. + \left( \frac{1}{2}R\rho + L + M \right) c^2 \right] \right\} \right], \\ \sigma_{yz} &\equiv \frac{d\beta}{dz} + \frac{d\gamma}{dy} = \frac{4(1+\eta)}{E} L y z, \end{aligned} \right\} \dots (6);$$

where E is Young's modulus,  $\eta$  Poisson's ratio.

A type of the three displacements is

$$\begin{aligned} \alpha &= \frac{x}{E} \left[ \left( \frac{1}{2}P\rho + M + N \right) a^2 - \eta \left( \frac{1}{2}Q\rho + N + L \right) b^2 \right. \\ &\quad \left. - \eta \left( \frac{1}{2}R\rho + L + M \right) c^2 \right. \\ &\quad - \frac{1}{3} \frac{x^2}{a^2} \left\{ \left( \frac{1}{2}P\rho + M + N \right) a^2 - \eta \left( \frac{1}{2}Q\rho + 3N + L \right) b^2 \right. \\ &\quad \left. - \eta \left( \frac{1}{2}R\rho + L + 3M \right) c^2 \right\} \\ &\quad - \frac{y^2}{b^2} \left\{ \left( \frac{1}{2}P\rho + M + 3N \right) a^2 - \eta \left( \frac{1}{2}Q\rho + N + L \right) b^2 \right. \\ &\quad \left. - \eta \left( \frac{1}{2}R\rho + 3L + M \right) c^2 \right\} \\ &\quad - \frac{z^2}{c^2} \left\{ \left( \frac{1}{2}P\rho + 3M + N \right) a^2 - \eta \left( \frac{1}{2}Q\rho + N + 3L \right) b^2 \right. \\ &\quad \left. - \eta \left( \frac{1}{2}R\rho + L + M \right) c^2 \right\} \right] \dots (7). \end{aligned}$$

The other strains and displacements can be written down from symmetry.

For the elastic increment  $\epsilon a$ , in a principal semi-axis  $a$ , we have

$$\delta a' a = \frac{2}{3E} [(\frac{1}{2}P\rho + M + N) a^2 - \eta \{ \frac{1}{2}\rho (Qb^2 + Rc^2) + L(b^2 + c^2) \}] \dots (8).$$

§ 2. Let  $t_x, \dots$  denote the component stresses across the tangent plane at  $x, y, z$  to a quadric concentric with, and similar and similarly situated to the bounding surface (1), for whose equation we take

$$a^{-2}x^2 + b^{-2}y^2 + c^{-2}z^2 = \lambda \dots\dots\dots (9),$$

then we easily find

$$\begin{aligned} t_x / \left\{ \frac{p_\lambda x}{\lambda a^2} (\frac{1}{2}P\rho + M + N) a^3 \right\} &= t_y / \left\{ \frac{p_\lambda y}{\lambda b^2} (\frac{1}{2}Q\rho + N + L) b^3 \right\} \\ &= t_z / \left\{ \frac{p_\lambda z}{\lambda c^2} (\frac{1}{2}R\rho + L + M) c^3 \right\} = 1 - \lambda \dots\dots\dots (10), \end{aligned}$$

where  $p_\lambda$  is the perpendicular from the centre on the tangent plane. Thus the resultant stresses across parallel tangent planes to the system (9) at the points of contact are all parallel, and their intensity varies as  $1 - \lambda$ .

§ 3. If the ellipsoid be rotating with uniform angular velocity  $\omega$  about the axis  $2a$ , we have

$$P = 0, \quad Q = R = \omega^2;$$

while if it be gravitating, the force between unit masses at unit distance being taken as unity,

$$P = 2\pi\rho abc \int_0^\infty \frac{du}{\sqrt{\{(a^2+u)^2(b^2+u)(c^2+u)\}}} \dots\dots\dots (11)*$$

with symmetrical expressions for  $Q$  and  $R$ . If the ellipsoid be gravitating, and at the same time rotating about a principal axis, we have only to add the respective values of  $P, Q, R$ . Substituting the values of  $P, Q, R$  in the expressions for  $L, M, N$ , and inserting the consequent values of  $L, M, N$  in the formulæ (4), (6), (7), we have the complete values of the stresses, strains, and displacements.

#### *Gravitating nearly Spherical Ellipsoid.*

§ 4. Denoting by  $\mu$  the force between two unit masses at unit distance, we may take

$$P = -\frac{4}{3}\pi\mu\rho \left( 1 - \frac{2a^2 - b^2 - c^2}{5a^2} \right),*$$

with symmetrical expressions for  $Q$  and  $R$ .

We thence find

\* Thomson and Tait's 'Natural Philosophy' vol. 1, Part II, p. 47.

$$\begin{aligned}
\widehat{xx} = \frac{2\pi\mu\rho^2a^2}{15(1-\eta)(7+5\eta)} & \left[ -\left(1-\frac{x^2}{a^2}\right) \left\{ (7+5\eta)(3-\eta) \right. \right. \\
& \quad \left. \left. - \frac{2a^2-b^2-c^2}{a^2} (2-\eta)(3+\eta) \right\} \right. \\
& + \frac{y^2}{b^2} \left\{ (7+5\eta)(1+3\eta) - \frac{a^2-b^2}{a^2} (8-3\eta-9\eta^2) \right. \\
& \quad \left. \left. - \frac{a^2-c^2}{a^2} (2+3\eta+15\eta^2) \right\} \right. \\
& + \frac{z^2}{c^2} \left\{ (7+5\eta)(1+3\eta) - \frac{a^2-b^2}{a^2} (2+3\eta+15\eta^2) \right. \\
& \quad \left. \left. - \frac{a^2-c^2}{a^2} (8-3\eta-9\eta^2) \right\} \right] \dots\dots (12),
\end{aligned}$$

$$\widehat{yz} = \frac{4\pi\mu\rho^2yz}{15(1-\eta)(7+5\eta)} \left\{ (7+5\eta)(1-2\eta) + \frac{2a^2-b^2-c^2}{a^2} (1-\eta-4\eta^2) \right\} \dots\dots (13).$$

The other stresses may be written down from symmetry.

With the notation of § 2,

$$t_x = -p_\lambda \frac{1-\lambda}{\lambda} \frac{2\pi\mu\rho^2x(3-\eta)}{15(1-\eta)} \left\{ 1 - \frac{2a^2-b^2-c^2}{a^2} \frac{(2-\eta)(3+\eta)}{(7+5\eta)(3-\eta)} \right\} \dots (14).$$

It will be noticed that

$$p_\lambda \frac{1-\lambda}{\lambda} x = px'(1-r^2/r'^2),$$

where  $r = \sqrt{(x^2+y^2+z^2)}$ ,  $r' = \sqrt{(x'^2+y'^2+z'^2)}$ ,

$x', y', z'$  being the coordinates of the point where the radius vector  $r$  produced cuts the surface of the ellipsoid, and  $p$  being the perpendicular from the centre on the tangent plane at  $x', y', z'$ . Near the surface we may put

$$1-r^2/r'^2 = 2(1-r/r'),$$

and so conclude that  $t_x$  and the other stress components *across* the tangent planes to  $(q)$  vary approximately as the distance from the surface.

As the stresses at the surface itself are of special interest in the event of any application to the earth, I shall briefly consider them for a spheroid in which  $b = a$ . The principal stresses are  $\widehat{nn}$ ,  $\widehat{tt}$ ,  $\widehat{\phi\phi}$ , directed respectively along the normal, the tangent to the meridian, and the perpendicular to the meridian. Using cylindrical coordinates,  $r, \phi, z$  where

$$r = \sqrt{(x^2+y^2)}, \quad \phi = \tan^{-1}y/x,$$

we get

$$\left. \begin{aligned} \widehat{nn} &= 0, \\ \widehat{tt} &= \frac{-4\pi\mu\rho^2a^3}{15(1-\eta)} \left\{ \frac{c^2}{p^2}(1-2\eta) + \frac{a^2-c^2}{a^2} \frac{1-\eta-4\eta^2}{7+5\eta} \right\}, \\ \widehat{\phi\phi} &= \frac{-4\pi\mu\rho^2a^3}{15(1-\eta)} \left\{ 1-2\eta + \frac{a^2-c^2}{a^2} \left( 1 - \frac{3r^2}{a^2} \right) \frac{1-\eta-4\eta^2}{7+5\eta} \right\} \end{aligned} \right\} \dots\dots (15)$$

where  $p$  is the perpendicular from the centre on the tangent plane.

On the "stress-difference" theory of rupture an objection to the application to the earth of the results obtained by applying the elastic solid theory to a perfect sphere, is that the surface values of  $\widehat{nn}-\widehat{\phi\phi}$  and  $\widehat{nn}-\widehat{tt}$  would, for ordinary values of  $\eta$ , be simply enormous.\* This objection, however, ceases to hold when the earth is treated as incompressible and truly spherical, because  $\widehat{tt}$  and  $\widehat{\phi\phi}$  then vanish, as well as  $\widehat{nn}$ . It is thus important to know what happens in the case of an incompressible material when the surface is slightly spheroidal. To do so, put  $\eta = \frac{1}{2}$  in (15), and we find

$$\left. \begin{aligned} \widehat{nn} &= 0, \\ \widehat{tt} &= 8\pi\mu\rho^2(a^2-c^2)/285, \\ \widehat{\phi\phi} &= 8\pi\mu\rho^2(a^2-c^2)(1-3r^2/a^2)/285 \end{aligned} \right\} \dots\dots\dots (16).$$

Over the surface the maximum stress-difference,  $\bar{S}$ , is the equatorial value of  $\widehat{tt}-\widehat{\phi\phi}$ , and is given by

$$\bar{S} = 8\pi\mu\rho^2(a^2-c^2)/95 \dots\dots\dots (17).$$

Substituting for  $\rho$ ,  $a$ ,  $c$  values suitable to the case of a homogeneous "earth," we find that approximately

$$\bar{S} = 9.4 \text{ tons weight per square inch} \dots\dots\dots (18).$$

This is large enough to show that even if the earth be supposed incompressible, the consequences of its mutual gravitation cannot safely be ignored.

The strains and displacements in the general case of gravitation in a nearly spherical ellipsoid may easily be deduced from (6) and (7). From the expressions for the changes in the lengths of the semi-axes we get

$$\frac{\delta a}{a} = -\frac{4}{15} \frac{\pi\mu\rho^2a^3}{E} \left\{ 1-2\eta - \frac{2a^2-b^2-c^2}{a^2} \frac{2(1-3\eta-3\eta^2)}{7+5\eta} \right\} \dots (19),$$

$$\frac{\delta b}{b} - \frac{\delta c}{c} = -\frac{4}{15} \pi\mu\rho^2(b^2-c^2) \frac{(1+\eta)(1+8\eta)}{E(7+5\eta)} \dots\dots\dots (20).$$

\* See 'Phil. Mag.,' Sept., 1891, p. 247.

The principal axes all shorten in any material which is not very nearly incompressible.

For absolutely incompressible material,

$$\frac{\delta a}{a} = -\frac{4}{57} \frac{\pi \mu \rho^2 a^3}{E} \frac{2a^2 - b^2 - c^2}{a^4} \dots\dots\dots (21);$$

thus a principal axis shortens or lengthens according as the square on it is greater or less than the arithmetic mean of the squares on the principal axes. In a spheroid  $b = a$ , we find from (21),

$$-\frac{\delta a}{a} = \frac{1}{3} \frac{\delta c}{c} = \frac{4}{57} \frac{\pi \mu \rho^2 (a^2 - c^2)}{E} = \frac{5}{6} \frac{\bar{S}}{E} \dots\dots\dots (22),$$

where  $\bar{S}$  is given by (17).

Taking the numerical value (18) for  $\bar{S}$ , and for  $E$  the high value  $20 \times 10^8$  grammes weight per square centimetre, we should get from (22) for a spheroid the shape and size of the earth, a shortening of some 5 miles in equatorial diameters and a lengthening of some 10 miles in the polar diameter relative to what these lengths would have been in the absence of gravitation.

#### *Rotating nearly Spherical Ellipsoid.*

§ 5. Suppose next that the nearly spherical ellipsoid rotates with uniform angular velocity  $\omega$  about  $2a$ . The values of  $\widehat{xx}$ ,  $\widehat{yy}$ ,  $\widehat{xy}$ , and  $\widehat{yz}$  are as follows:—

$$\begin{aligned} \widehat{xx} = & \frac{-\omega^2 \rho a^3}{5(1-\eta)(7+5\eta)^2} \left[ \left( 1 - \frac{x^2}{a^2} \right) \left\{ (7+5\eta)(3-6\eta-5\eta^2) \right. \right. \\ & \left. \left. - \frac{2a^2-b^2-c^2}{a^2} 2(1+\eta)(6-5\eta-5\eta^2) \right\} \right. \\ & - \frac{y^2}{b^2} \left\{ 2(7+5\eta)(3-6\eta-5\eta^2) - \frac{a^2-b^2}{a^2} (39+16\eta-29\eta^2-10\eta^3) \right. \\ & \left. \left. - \frac{a^2-c^2}{a^2} (9-8\eta-51\eta^2-30\eta^3) \right\} \right. \\ & \left. - \frac{z^2}{c^2} \left\{ 2(7+5\eta)(3-6\eta-5\eta^2) - \frac{a^2-b^2}{a^2} (9-8\eta-51\eta^2-30\eta^3) \right. \right. \\ & \left. \left. - \frac{a^2-c^2}{a^2} (39+16\eta-29\eta^2-10\eta^3) \right\} \right] \dots\dots\dots (23), \end{aligned}$$

$$\begin{aligned} \widehat{yy} = & \frac{\omega^2 \rho b^3}{5(1-\eta)(7+5\eta)^2} \left[ -\frac{x^2}{a^3} \left\{ (7+5\eta)(9+7\eta) \right. \right. \\ & + \frac{a^2-b^2}{a^2} (39+23\eta-3\eta^2+5\eta^3) - \frac{a^2-c^2}{a^2} (6+13\eta+36\eta^2+25\eta^3) \Big\} \\ & + \left( 1 - \frac{y^2}{b^2} \right) \left\{ (7+5\eta)(12+\eta-5\eta^2) + \frac{a^2-b^2}{a^2} (12+9\eta+6\eta^2+5\eta^3) \right. \\ & \left. \left. - \frac{a^2-c^2}{a^2} (1+\eta)(3+5\eta^2) \right\} \right. \\ & \left. - \frac{x^2}{c^2} \left\{ (7+5\eta)(4+7\eta+5\eta^2) + \frac{a^2-b^2}{a^2} (9+13\eta+27\eta^2+15\eta^3) \right. \right. \\ & \left. \left. - \frac{a^2-c^2}{a^2} (6-\eta-16\eta^2-6\eta^3) \right\} \right] \dots\dots (24), \end{aligned}$$

$$\begin{aligned} \widehat{xy} = & \frac{-\omega^2 \rho xy}{5(1-\eta)(7+5\eta)^2} \left[ (7+5\eta)(3-6\eta-5\eta^2) \right. \\ & \left. - \frac{a^2-b^2}{a^2} (27+14\eta-9\eta^2) + \frac{a^2-c^2}{a^2} (3+10\eta+31\eta^2+20\eta^3) \right] \\ & \dots\dots (25), \end{aligned}$$

$$\begin{aligned} \widehat{yz} = & \frac{-\omega^2 \rho yz}{5(1-\eta)(7+5\eta)^2} \left[ 2(7+5\eta)(4-3\eta-5\eta^2) \right. \\ & \left. + \frac{2a^2-b^2-c^2}{a^2} (3-4\eta-21\eta^2-10\eta^3) \right] \dots\dots (26). \end{aligned}$$

$\widehat{zx}$  may be obtained from  $\widehat{yy}$ , and  $\widehat{zx}$  from  $\widehat{xy}$  by interchanging  $y$  with  $z$  and  $b$  with  $c$ .

The reduction in length along the axis of rotation is given by

$$\begin{aligned} (-\delta a/a) = & \frac{2\omega^2 \rho a^3}{5E(7+5\eta)} \left[ 1+8\eta+5\eta^3 \right. \\ & \left. - \frac{2a^2-b^2-c^2}{a^2} \frac{24+223\eta+282\eta^2+95\eta^3}{6(7+5\eta)} \right] \dots\dots (27). \end{aligned}$$

It is thus greater or less than in a sphere of radius  $a$ , according as  $a^2$  is less or greater than the arithmetic mean of the squares on the principal semi-axes. The tendency to shorten in the axis of rotation increases as the perpendicular diameters lengthen.

For the increment in a perpendicular semi-axis  $b$ , we get

$$\begin{aligned} \delta b/b = & \frac{\omega^2 \rho a^3}{5E(7+5\eta)} \left[ 8-\eta-5\eta^2 + \frac{b^2+c^2-2a^2}{a^2} \frac{25+16\eta-15\eta^2-10\eta^3}{7+5\eta} \right. \\ & \left. + \frac{b^2-c^2}{a^2} \frac{(1+\eta)(69+137\eta+70\eta^2)}{3(7+5\eta)} \right] \dots\dots (28). \end{aligned}$$

From this we see that the "extension per unit of length"\* is greatest in the longer of the two principal diameters perpendicular to the axis of rotation.

*Very Flat Ellipsoid.*

§ 6. Supposing  $2c$  the short axis, we find, neglecting higher powers of  $c/a$  and  $b/a$ ,

$$\begin{aligned} \widehat{xx} = \frac{1}{3}\rho \left[ Pa^2 \left( 1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} \right) + Rc^2 \frac{\eta}{2(1-\eta)} \left( 1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} - 3 \frac{z^2}{c^2} \right) \right. \\ \left. + \frac{Pa^2(\eta b^2 - a^2) + Qb^2(\eta a^2 - b^2) + Rc^2\eta(a^2 + b^2)}{4a^4 + 3a^2b^2 + 4b^4 + \eta a^2b^2} a^2 \left( 1 - \frac{x^2}{a^2} - \frac{4y^2}{b^2} \right) \right] \\ \dots (29), \end{aligned}$$

$$\begin{aligned} \widehat{zz} = \frac{1}{6}\rho c^2 \left[ -P \left( 1 - \frac{3x^2}{a^2} - \frac{y^2}{b^2} - \frac{z^2}{c^2} \right) - Q \left( 1 - \frac{x^2}{a^2} - \frac{3y^2}{b^2} - \frac{z^2}{c^2} \right) \right. \\ \left. + 3R \left( 1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} - \frac{z^2}{c^2} \right) \right. \\ \left. - 2 \frac{Pa^2(\eta b^2 - a^2) + Qb^2(\eta a^2 - b^2)}{4a^4 + 3a^2b^2 + 4b^4 + \eta a^2b^2} \left( 1 - \frac{2x^2}{a^2} - \frac{2y^2}{b^2} - \frac{z^2}{c^2} \right) \right] \dots (30), \end{aligned}$$

$$\widehat{xy} = \rho xy \frac{\{ Pa^2(\eta b^2 - a^2) + Qb^2(\eta a^2 - b^2) + Rc^2\eta(a^2 + b^2) \}}{4a^4 + 3a^2b^2 + 4b^4 + \eta a^2b^2} \dots (31),$$

$$\begin{aligned} \widehat{xz} = \frac{1}{3}\rho xz \left\{ -P + \frac{\eta}{1-\eta} \frac{Rc^2}{a^2} \right. \\ \left. - \frac{Pa^2(\eta b^2 - a^2) + Qb^2(\eta a^2 - b^2) + Rc^2\eta(a^2 + b^2)}{4a^4 + 3a^2b^2 + 4b^4 + \eta a^2b^2} \right\} \dots (32). \end{aligned}$$

$\widehat{yy}$  is got from  $\widehat{xx}$ , and  $\widehat{yz}$  from  $\widehat{xz}$  by interchanging  $P$  with  $Q$  and  $a$  with  $b$ .

The forces  $P, Q, R$  may occur independently, so the most important term in the coefficient of each has been retained.

When  $P$  and  $Q$  alone exist, or the forces are perpendicular to the small dimension,  $\widehat{xx}$  and  $\widehat{yz}$  bear to  $\widehat{xx}$ ,  $\widehat{yy}$ , and  $\widehat{xy}$  ratios of the order  $c:a$ , while  $\widehat{zz}$  bears a ratio of the order  $c^2:a^2$ . Thus for a first approximation the three stresses  $\widehat{xx}$ ,  $\widehat{yy}$ , and  $\widehat{xy}$  alone need be retained. To a first approximation  $\widehat{xx}$ ,  $\widehat{yy}$ , and  $\widehat{xy}$  are constant along any line parallel to the small dimension. These conclusions

\* Used here and subsequently in sense of total change in length of axis divided by original length, whether strain be uniform or not along the axis.

are so far in harmony with the assumptions usually made in theories of thin plates. It should be noticed, however, that  $\widehat{dxx}/dz$  is of the same order of magnitude as  $\widehat{dxx}/dx$ , and so of equal importance in the first body-stress equation.

When  $R$  alone exists, or the forces are parallel to the small dimension,  $\widehat{zz}$  at points in the interior is of the same order of magnitude as  $\widehat{xx}$ ,  $\widehat{yy}$ , and  $\widehat{xy}$ .

If there be bodily forces both parallel and perpendicular to the small dimension, and the two sets be of the same order of magnitude, then to a first approximation the stresses due to the former set of forces may be neglected.

### Gravitating very Oblate Spheroid.

§7. Taking  $\mu$  as before for the gravitation constant, we have as a first approximation \*

$$P = Q = -\pi^2 \mu \rho c/a, \quad R = -4\pi \mu \rho \dots\dots\dots (33).$$

As there is symmetry round the axis of  $z$ , we employ cylindrical coordinates  $r, \phi, z$ . The notation  $\widehat{rr}, \widehat{\phi\phi}, \dots$  for the stresses explains itself. The displacements are  $u$  along  $r$ , and  $\gamma$  parallel to  $z$ . The strains are

$$\left. \begin{aligned} s_r &= du/dr, & s_\phi &= u/r, & s_z &= d\gamma/dz, \\ \sigma_{r\phi} &= 0, & \sigma_{\phi z} &= 0, & \sigma_{rz} &= du/dz + d\gamma/dr \end{aligned} \right\} \dots\dots (34).$$

For the values of the stresses we find

$$\left. \begin{aligned} \widehat{rr} &= -\pi^2 \mu \rho^2 a c \frac{3+\eta}{11+\eta} \left(1 - \frac{r^2}{a^2}\right), \\ \widehat{\phi\phi} &= -\frac{\pi^2 \mu \rho^2 a c}{11+\eta} \left\{ 3+\eta - (1+3\eta) \frac{r^2}{a^2} \right\}, \\ \widehat{zz} &= -2\pi \mu \rho^2 c^2 (1 - r^2/a^2 - z^2/c^2), \\ \widehat{rz} &= \pi^2 \mu \rho^2 \frac{3+\eta}{11+\eta} \frac{c}{a} zr, \\ \widehat{r\phi} &= \widehat{\phi z} = 0 \end{aligned} \right\} \dots\dots\dots (35).$$

The strains which differ from zero are

\* Thomson and Tait's 'Natural Philosophy,' vol. 1, Part II, Art. 527.

$$\left. \begin{aligned} s_r &= -\frac{\pi^2 \mu \rho^2 a c (1-\eta)}{E(11+\eta)} \left\{ 3+\eta-3(1+\eta) \frac{r^2}{a^2} \right\}, \\ s_\phi &= -\frac{\pi^2 \mu \rho^2 a c (1-\eta)}{E(11+\eta)} \left\{ 3+\eta-(1+\eta) \frac{r^2}{a^2} \right\}, \\ s_z &= \frac{2\pi^2 \mu \rho^2 a c \eta}{E(11+\eta)} \left\{ 3+\eta-2(1+\eta) \frac{r^2}{a^2} \right\}, \\ \sigma_{rr} &= 2\pi^2 \mu \rho^2 \frac{c}{a} z r \frac{(1+\eta)(3+\eta)}{E(11+\eta)} \end{aligned} \right\} \dots\dots (36).$$

The displacements and dilatation are

$$\left. \begin{aligned} u &= -\frac{\pi^2 \mu \rho^2 a c r (1-\eta)}{E(11+\eta)} \left\{ 3+\eta-(1+\eta) \frac{r^2}{a^2} \right\}, \\ \gamma &= \frac{2\pi^2 \mu \rho^2 a c z \eta}{E(11+\eta)} \left\{ 3+\eta-2(1+\eta) \frac{r^2}{a^2} \right\}, \\ \Delta &= -\frac{2\pi^2 \mu \rho^2 a c (1-2\eta)}{E(11+\eta)} \left\{ 3+\eta-2(1+\eta) \frac{r^2}{a^2} \right\} \end{aligned} \right\} \dots (37).$$

Strictly,  $\widehat{zz}$ ,  $\widehat{zr}$ ,  $\sigma_{rr}$  and  $\gamma$ , being of the order of terms omitted in  $\widehat{rr}$ ,  $s_r$ , &c., should be neglected; but the record of their first approximations may prove useful.

$\widehat{rr}$ ,  $\widehat{\phi\phi}$  and  $\widehat{zz}$  are pressures at every interior point;  $s_\phi$  is everywhere a compression, and  $s_z$  an extension, while  $s_r$  is a compression inside and an extension outside the cylinder

$$r = r_1 = a \sqrt{\left\{ \frac{1}{3} (3+\eta) / (1+\eta) \right\}} \dots\dots\dots (38).$$

The displacement  $u$  is everywhere towards the axis of the spheroid, and  $\gamma$  away from the central plane,  $z = 0$ . Every element is reduced in volume.

*Flat Ellipsoid Rotating about the Short Axis  $2c$ .*

§ 8. Here

$$P = Q = \omega^2, \quad R = 0.$$

Putting for shortness

$$4a^4 + 3a^2b^2 + 4b^4 + \eta a^2b^2 = \Pi' \dots\dots\dots (39),$$

we find

$$\left. \begin{aligned} \widehat{xx} &= \omega^2 \rho a^2 [\{a^4 + a^2 b^2 (1 + \eta) + b^4\} (1 - x^2/a^2) \\ &\quad - (1 + 3\eta) a^2 y^2] \div \Pi', \\ \widehat{yy} &= \omega^2 \rho b^2 [-(1 + 3\eta) b^2 x^2 \\ &\quad + \{a^4 + a^2 b^2 (1 + \eta) + b^4\} (1 - y^2/b^2)] \div \Pi', \\ \widehat{zz} &= -\omega^2 \rho c^2 \{a^4 + a^2 b^2 (1 + \eta) + b^4\} \\ &\quad \times \{1 - 2x^2/a^2 - 2y^2/b^2 - z^2/c^2\} \div \Pi', \\ \widehat{xy} &= -\omega^2 \rho (a^4 - 2\eta a^2 b^2 + b^4) xy \div \Pi', \\ \widehat{yz}/yz &= \widehat{zx}/zx = -\omega^2 \rho \{a^4 + a^2 b^2 (1 + \eta) + b^4\} \div \Pi' \end{aligned} \right\} \dots (40).$$

$\widehat{zx}$ , it should be noticed, is negligible compared to  $\widehat{yz}$  and  $\widehat{xx}$ , and these in turn negligible compared to  $\widehat{xx}$ ,  $\widehat{yy}$  and  $\widehat{xy}$ . The values of  $\widehat{xx}$ ,  $\widehat{yy}$ , and  $\widehat{xy}$  bear to the corresponding first approximations in the case of an elliptic disc\* of semi-axes  $a$  and  $b$ , rotating about its cylindrical axis with the same angular velocity  $\omega$ , the common ratio

$$3a^4 + 2a^2b^2 + 3b^4 : 4a^4 + (3 + \eta) a^2b^2 + 4b^4 \dots \dots \dots (41).$$

This ratio also applies to the first approximations to  $\Delta$ ,  $s_x$ ,  $s_y$ ,  $s_z$ , and  $\sigma_{xy}$ ; while the other strains are in both cases negligible compared to these. It applies further to  $\delta a/a$  and  $\delta b/b$ . Thus the great majority of the results worked out for the rotating disc can be simply modified so as to apply to the flat ellipsoid by means of the following table which proceeds to three places of decimals.

Table I.—Values of  $(3a^4 + 2a^2b^2 + 3b^4) \div \{4a^4 + (3 + \eta) a^2b^2 + 4b^4\}$ .

| $\eta$ | $b/a$ | 0.   | 0.2   | 0.4   | 0.6   | 0.8   | 1.0   |
|--------|-------|------|-------|-------|-------|-------|-------|
| 0      | .     | 0.75 | 0.748 | 0.741 | 0.734 | 0.729 | 0.72  |
| 0.25   |       | 0.75 | 0.746 | 0.735 | 0.722 | 0.714 | 0.71  |
| 0.5    |       | 0.75 | 0.744 | 0.729 | 0.711 | 0.699 | 0.696 |

On either the "greatest strain" or the "maximum stress-difference" theory of rupture, the limiting angular velocity in the flat ellipsoid

\* 'Phil. Mag.,' July, 1892, pp. 70—100; see results (17), (18), (19), p. 75.

bears to that in the thin disc, of the same material and axes, a ratio which is the square root of the reciprocal of (41).

If the limiting angular velocities be  $\omega$  for the ellipsoid,  $\omega'$  for the disc, then the approximate values of  $\omega/\omega'$  when  $\eta = \cdot 25$ , are as follows:—

Table II.

|                    |       |       |       |       |       |       |
|--------------------|-------|-------|-------|-------|-------|-------|
| $b/a =$            | 0     | 0.2   | 0.4   | 0.6   | 0.8   | 1.0   |
| $\omega/\omega' =$ | 1.155 | 1.158 | 1.167 | 1.177 | 1.184 | 1.186 |

In the case of the thin disc many very simple results were found for the radial strains; these apply without modification to the strain  $s_r$  in the flat ellipsoid.

*Flat Ellipsoid rotating about one of its longer axes  $2a$ .*

§ 9. Putting  $P = 0$ ,  $Q = R = \omega^2$ , and employing  $\Pi'$  as in (39), we find

$$\left. \begin{aligned} \widehat{xx} &= \frac{1}{3\Pi'} \omega^2 \rho a^2 b^2 (\eta a^2 - b^2) \left( 1 - \frac{x^2}{a^2} - \frac{4y^2}{b^2} \right), \\ \widehat{yy} &= \frac{1}{3} \omega^2 \rho b^2 \left\{ 1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} + \frac{b^2 (\eta a^2 - b^2)}{\Pi'} \left( 1 - \frac{4x^2}{a^2} - \frac{y^2}{b^2} \right) \right\}, \\ \widehat{zz} &= \frac{1}{3} \omega^2 \rho c^2 \left\{ 1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} - \frac{b^2 (\eta a^2 - b^2)}{\Pi'} \left( 1 - \frac{2x^2}{a^2} - \frac{2y^2}{b^2} - \frac{z^2}{c^2} \right) \right\}, \\ \widehat{yz} &= -\frac{1}{3} \omega^2 \rho yz \left\{ 1 + \frac{b^2 (\eta a^2 - b^2)}{\Pi'} \right\}, \\ \widehat{zx} &= -\frac{1}{3} \frac{\omega^2 \rho b^2}{\Pi'} (\eta a^2 - b^2) zx, \\ \widehat{xy} &= \frac{\omega^2 \rho b^2}{\Pi'} (\eta a^2 - b^2) xy \end{aligned} \right\} \dots (42).$$

As  $b/a$  passes through the critical value  $\sqrt{\eta}$ , the stresses  $\widehat{xx}$ ,  $\widehat{zx}$ , and  $\widehat{xy}$  vanish and change sign; and at the critical value, to a first approximation, the only stress is

$$\widehat{yy} = \frac{1}{3} \omega^2 \rho b^2 (1 - x^2/a^2 - y^2/b^2) \dots \dots \dots (43)$$

The stretches answering to (42) are

$$\left. \begin{aligned} s_x &= -\frac{1}{3} \frac{\omega^2 \rho b^3}{E} \left[ \left\{ \eta + \frac{(a^2 - \eta b^2)(b^2 - \eta a^2)}{\Pi'} \right\} \left( 1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} \right) \right. \\ &\quad \left. + 3 \frac{(b^2 - \eta a^2)}{\Pi'} \left( \eta \frac{b^2}{a^2} x^2 - \frac{a^2}{b^2} y^2 \right) \right], \\ s_y &= \frac{1}{3} \frac{\omega^2 \rho b^3}{E} \left[ \left\{ 1 - \frac{(b^2 - \eta a^2)^2}{\Pi'} \right\} \left( 1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} \right) \right. \\ &\quad \left. + \frac{3(b^2 - \eta a^2)}{\Pi'} \left( \frac{b^2}{a^2} x^2 - \eta \frac{a^2}{b^2} y^2 \right) \right], \\ s_z &= -\frac{1}{3} \frac{\omega^2 \rho b^2 \eta}{E} \left[ \left\{ 1 + \frac{(\eta a^2 - b^2)(a^2 + b^2)}{\Pi'} \right\} \left( 1 - \frac{x^2}{a^2} - \frac{y^2}{b^2} \right) \right. \\ &\quad \left. + \frac{3(b^2 - \eta a^2)}{\Pi'} \left( \frac{b^2}{a^2} x^2 + \frac{a^2}{b^2} y^2 \right) \right] \end{aligned} \right\} \dots (44).$$

For the increments in the semi-axes we have

$$\left. \begin{aligned} \frac{\delta a}{a} &= -\frac{1}{3} \frac{\omega^2 \rho b^3}{E} \{ 2\eta + (b^2 - \eta a^2)(2a^2 + \eta b^2)/\Pi' \}, \\ \frac{\delta b}{b} &= \frac{1}{3} \frac{\omega^2 \rho b^3}{E} \{ 2 - (b^2 - \eta a^2)(\eta a^2 + 2b^2)/\Pi' \}, \\ \frac{\delta c}{c} &= -\frac{1}{3} \frac{\omega^2 \rho b^2 \eta}{E} \{ 1 - (b^2 - \eta a^2)(a^2 + b^2)/\Pi' \} \end{aligned} \right\} \dots (45).$$

It is easy to prove  $\delta b/b$  positive, and  $\delta c/c$  negative, for all values of  $\eta$  and  $b/a$ ; thus, of the axes perpendicular to the axis of rotation, the longer lengthens and the shorter shortens. The axis  $2a$  of rotation is always reduced.

When the flat ellipsoid is spheroidal ( $-\delta c/c$ ) is greater or less than ( $-\delta a/a$ ), according as  $\eta$  is greater or less than .25.

#### Very Elongated Ellipsoid.

§ 10. Treating  $a/c$  and  $b/c$  as both very small, we find

$$\begin{aligned} & \widehat{xx} \cdot 4(1 - \eta^2)(3a^4 + 2a^2b^2 + 3b^4) \\ &= 2P\rho a^2(1 + \eta) \left[ \{ (1 - \eta)(2a^4 + 2a^2b^2 + 3b^4) + \eta a^2b^2 \} \left( 1 - \frac{x^2}{a^2} - \frac{y^2}{c^2} \right) \right. \\ &\quad \left. - \{ 2a^2 + 3b^2 + \eta(a^2 - 3b^2) \} y^2 \right] \\ &\quad + 2Q\rho b^2a^2(1 + \eta) \{ \eta(a^2 + b^2) - b^2 \} \left\{ 1 - \frac{x^2}{a^2} - \frac{3y^2}{b^2} - \frac{z^2}{c^2} \right\} \end{aligned}$$

$$-R\rho a^2 \left[ \{ (1-\eta^2) (a^4 + a^2b^2 + b^4) - \eta (1+3\eta) a^2b^2 \} \left( 1 - \frac{x^2}{a^2} \right) - a^2b^2 (1+2\eta) (1-5\eta) \frac{y^2}{b^2} - \{ (1-\eta^2) (4a^4 + 3a^2b^2 + 4b^4) - \eta (1+3\eta) a^2b^2 \} \frac{z^2}{c^2} \right] \dots (46),$$

$$\begin{aligned} & \widehat{zz} (1-\eta^2) \left[ 4 (3a^4 + 2a^2b^2 + 3b^4) + \frac{a^2+b^2}{c^2} \{ (3+\eta) (3a^4 + 2a^2b^2 + 3b^4) + 8\eta a^2b^2 \} \right] \\ &= P\rho a^2\eta \left[ -2\{3a^4 + 2a^2b^2 + 3b^4 - (a^2 - \eta b^2) (a^2 + 3b^2) - 2\eta^2 a^2 (a^2 - b^2)\} \frac{x^2}{a^2} \right. \\ &\quad - 2\{3a^4 + 2a^2b^2 + 3b^4 - (a^2 - \eta b^2) (3a^2 + b^2) + 2\eta^2 b^2 (a^2 - b^2)\} \frac{y^2}{b^2} \\ &\quad \left. + \{3a^4 + 2a^2b^2 + 3b^4 - 2(a^2 - \eta b^2) (a^2 + b^2) - \eta^2 (a^2 - b^2)^2\} \left( 1 - \frac{z^2}{c^2} \right) \right] \\ &+ Q\rho b^2\eta \left[ -2\{3a^4 + 2a^2b^2 + 3b^4 - (b^2 - \eta a^2) (a^2 + 3b^2) - 2\eta^2 a^2 (a^2 - b^2)\} \frac{x^2}{a^2} \right. \\ &\quad - 2\{3a^4 + 2a^2b^2 + 3b^4 - (b^2 - \eta a^2) (3a^2 + b^2) + 2\eta^2 b^2 (a^2 - b^2)\} \frac{y^2}{b^2} \\ &\quad \left. + \{3a^4 + 2a^2b^2 + 3b^4 - 2(b^2 - \eta a^2) (a^2 + b^2) - \eta^2 (a^2 - b^2)^2\} \left( 1 - \frac{z^2}{c^2} \right) \right] \\ &+ R\rho c^2 \left[ -\{3a^4 + 2a^2b^2 + 3b^4 + \eta(5a^4 + 3a^2b^2 + 4b^4) - \eta^2 a^2 (3a^2 + b^2) - 5\eta^2 (a^4 - b^4)\} \frac{x^2}{c^2} \right. \\ &\quad - \{3a^4 + 2a^2b^2 + 3b^4 + \eta(4a^4 + 3a^2b^2 + 5b^4) - \eta^2 b^2 (a^2 + 3b^2) + 5\eta^2 (a^4 - b^4)\} \frac{y^2}{c^2} \\ &\quad \left. + \{ (1-\eta^2) (3a^4 + 2a^2b^2 + 3b^4) + \frac{a^2+b^2}{c^2} \left( 3a^4 + 2a^2b^2 + 3b^4 + \eta(2a^4 + 3a^2b^2 + 2b^4) \right. \right. \\ &\quad \left. \left. - \eta^2 (3a^4 + a^2b^2 + 3b^4) - 2\eta^3 (a^4 + b^4) \right) \right\} \left( 1 - \frac{z^2}{c^2} \right) \right] \dots \dots (47), \\ &\widehat{xy} . 4 (1-\eta^2) (3a^4 + 2a^2b^2 + 3b^4) \end{aligned}$$

$$= \rho xy [4(1+\eta) P a^2 \{ \eta (a^2 + b^2) - a^2 \} + 4(1+\eta) Q b^2 \{ \eta (a^2 + b^2) - b^2 \} + R \{ (1-\eta^2) (a^4 + b^4) + 2\eta (1+3\eta) a^2b^2 \}] \dots (48),$$

$$\begin{aligned} & \widehat{zx} (1-\eta^2) \left[ 4 (3a^4 + 2a^2b^2 + 3b^4) + \frac{a^2+b^2}{c^2} \{ (3+\eta) (3a^4 + 2a^2b^2 + 3b^4) + 8\eta a^2b^2 \} \right] \\ &= \rho zx \left[ P \frac{a^2}{c^2} \eta \{ 3a^4 + 2a^2b^2 + 3b^4 - 4b^2 (a^2 - \eta b^2) - \eta^2 (a^2 - b^2) (3a^2 + b^2) \} \right. \\ &\quad + Q \frac{b^2}{c^2} \eta \{ 3a^4 + 2a^2b^2 + 3b^4 - 4b^2 (b^2 - \eta a^2) - \eta^2 (a^2 - b^2) (3a^2 + b^2) \} \\ &\quad \left. - R \left\{ (1-\eta^2) (3a^4 + 2a^2b^2 + 3b^4) + \frac{1}{c^2} \left( b^2 (3a^4 + 2a^2b^2 + 3b^4) \right. \right. \right. \\ &\quad \left. \left. - \eta (3a^4 - 2a^2b^2 - a^2b^4 - 2b^4) - \eta^2 b^2 (3a^4 + 4a^2b^2 + 3b^4) \right. \right. \\ &\quad \left. \left. + \eta^3 (a^2 + b^2) (a^2 - 2b^2) (3a^2 + b^2) \right) \right\} \right] \dots (49). \end{aligned}$$

$\widehat{yy}$  and  $\widehat{yz}$  may be deduced from  $\widehat{xx}$  and  $\widehat{zx}$  respectively by interchanging P with Q,  $x$  with  $y$ , and  $a$  with  $b$ .

If P, Q, and R be of the same order of magnitude, the principal terms depending on them in the values of  $\widehat{xx}$ ,  $\widehat{yy}$ , and  $\widehat{xy}$  are likewise of the same order; but in  $\widehat{zz}$ ,  $\widehat{yz}$ , and  $\widehat{zx}$ , the principal terms in P and Q are only of the same order as the secondary terms in R. I have thus thought it best, in (47) and (49), to retain secondary terms in the coefficients of R, and to write the second approximation value of II. If, however, R be zero, the terms on the left-hand sides of these equations with  $c^2$  in the denominator should be dropped.

When R alone exists, or the bodily forces are parallel to the long dimension, then, except near the ends of the long axis,  $\widehat{zz}$  is large compared to  $\widehat{yz}$  and  $\widehat{zx}$ , while these in their turn are, at most points, large compared to  $\widehat{xx}$ ,  $\widehat{yy}$ , and  $\widehat{xy}$ . The hypothesis usually made in treating long rods, viz.:—

$$\widehat{xx} = \widehat{yy} = \widehat{xy} = 0 \dots\dots\dots (50),$$

is thus approximately true.  $d\widehat{xx}/dx$  is, however, of the same order as  $d\widehat{zx}/dx$ , and  $d\widehat{zx}/dx$  of the same order as  $d\widehat{zz}/dx$ , so that the neglect of any of the nine differential coefficients appearing in the body-stress equations would be unjustifiable.\*

When the bodily forces are perpendicular to the long dimension, then—excluding special values of  $x$ ,  $y$ ,  $z$ — $\widehat{xx}$ ,  $\widehat{yy}$ ,  $\widehat{zz}$  and  $\widehat{xy}$  are of the same order of magnitude, and are large compared to  $\widehat{yz}$  and  $\widehat{zx}$ . This result differs widely from (50).

When P, Q, R are of the same order of magnitude, we may, for a rough first approximation, neglect all the stresses but  $\widehat{zz}$ , and take

$$\left. \begin{aligned} \widehat{zz} &= \frac{1}{4}R\rho(c^2 - z^2), \\ -s_x/\eta &= -s_y/\eta = s_z = \frac{1}{4}(R\rho/E)(c^2 - z^2), \\ \alpha/x &= \beta/y = -\frac{1}{4}(R\rho\eta/E)(c^2 - z^2), \\ \gamma &= \frac{1}{4}(R\rho/E)z(c^2 - \frac{1}{3}z^2), \\ \delta a/u &= \delta b/b = -\frac{1}{4}R\rho\eta c^2/E, \\ \delta c/c &= \frac{1}{8}R\rho c^2/E \end{aligned} \right\} \dots\dots\dots (51).$$

The strain at any point is the same as in a long bar subjected to a tension at its ends equal per unit of section to the local value of  $\widehat{zz}$ .

\* See Todhunter and Pearson's 'History,' vol. 2, Part II, pp. 189—191.

To a closer degree of approximation we have, for the stress system,

$$-\widehat{zx}/zx = -\widehat{yz}/yz = \widehat{zz}/(c^2 - z^2) = \frac{1}{4}R\rho \dots\dots\dots (52),$$

and for the strain system, in addition to  $s_x$ ,  $s_y$ , and  $s_z$ , in (51),

$$\sigma_{xz}/zx = \sigma_{yz}/yz = -\frac{1}{4}R\rho/n \dots\dots\dots (53),$$

where  $n$  is the rigidity.

An analysis of (52) gives, on a plane perpendicular to  $z$ , a stress  $\frac{1}{4}R\rho(c^2 - z^2)$  parallel to  $z$ , and a shearing stress  $-\frac{1}{4}R\rho xz$  along  $x$ , the perpendicular on the axis of  $z$ ; on a plane perpendicular to  $x$  a shearing stress,  $-\frac{1}{4}R\rho xz$ , parallel to  $z$ ; on any plane containing the axis of  $z$ , no stress.

Even in the general case with  $P$ ,  $Q$ ,  $R$  all existent, and secondary terms retained in the coefficient of  $R$  and in  $\Pi$ , we get for  $s_z$  the simple formula

$$s_z = \frac{\rho}{4E} \left[ -\eta \frac{Pa^2 + Qb^2}{c^2} (c^2 - z^2) + R \left\{ \left( 1 + (1 + 7\eta) \frac{a^2 + b^2}{4c^2} \right) (c^2 - z^2) - (1 + 2\eta) (x^2 + y^2) - \eta(a^2 + b^2) \right\} \right] \dots (54).$$

When the bodily forces are perpendicular to the long axis, the stretch parallel to that axis is thus appreciably constant over a cross section; these perpendicular forces tend to shorten or to lengthen the long axis according as they act outwards from it or towards it.

*Elongated Ellipsoid Rotating about the Long Axis  $2c$ .*

§ 11. Putting in (46) to (49)

$$P = Q = \omega^2, \quad R = 0,$$

we get

$$\widehat{xx} = \frac{\omega^2 \rho a^2}{(1 - \eta)(3a^4 + 2a^2b^2 + 3b^4)} \left[ \{a^4 + a^2b^2 + b^4 - \eta(a^4 + b^4)\} \left( 1 - \frac{x^2}{a^2} - \frac{z^2}{c^2} \right) - (1 + 2\eta)a^2y^2 \right] \dots (55),$$

$$\begin{aligned} \widehat{zz} = & \frac{-\omega^2 \rho \eta}{(1 - \eta)(3a^4 + 2a^2b^2 + 3b^4)} \left[ \{a^4 + a^2b^2 + 2b^4 - \eta(a^4 - b^4)\} x^2 \right. \\ & \left. + \{2a^4 + a^2b^2 + b^4 + \eta(a^4 - b^4)\} y^2 \right. \\ & \left. - \frac{1}{4}(a^2 + b^2)\{(a^2 + b^2)^2 - \eta(a^2 - b^2)^2\} \left( 1 - \frac{z^2}{c^2} \right) \right] \dots (56), \end{aligned}$$

$$\widehat{xy} = \frac{\omega^2 \rho \{ \eta (a^2 + b^2)^2 - a^4 - b^4 \} xy}{(1 - \eta) (3a^4 + 2a^2b^2 + 3b^4)} \dots\dots\dots (57),$$

$$\widehat{zx} = \frac{\omega^2 \rho \eta z x}{4(1 - \eta^2) c^2 (3a^4 + 2a^2b^2 + 3b^4)} [(a^2 + b^2) (3a^4 + 2a^2b^2 + 3b^4) - 4b^2(a^4 - 2\eta a^2b^2 + b^4) - \eta^2(a^2 - b^2)(a^2 + b^2)(3a^2 + b^2)] \dots (58).$$

$\widehat{yy}$  and  $\widehat{yz}$  may be got from  $\widehat{xx}$  and  $\widehat{zx}$  respectively by interchanging  $x$  with  $y$  and  $a$  with  $b$ . To a first approximation  $\widehat{yz}$  and  $\widehat{zx}$  are negligible.

Neglecting  $z^2/c^2$  in these formulæ, we obtain results applicable to the central portion of the long ellipsoid; these results are identical with those I have previously obtained for a long elliptic cylinder,\* the axes of whose elliptic section are  $2a$  and  $2b$ , rotating about the cylindrical axis. To deduce results for the elongated ellipsoid from those found for the infinite cylinder, we write  $1 - z^2/c^2$  for 1 in the constant terms in  $s_x$ ,  $s_y$ ,  $s_z$ , and  $\Delta$ , in the coefficient of  $x$  in  $\alpha$  and in that of  $y$  in  $\beta$ ; and we multiply the expression for  $\gamma$  by  $1 - \frac{1}{3}z^2/c^2$ .

The strain and displacement parallel to the long axis are of special interest; they are

$$s_x = -\frac{1}{4} \frac{\omega^2 \rho \eta (a^2 + b^2)}{E} \left( 1 - \frac{z^2}{c^2} \right) \dots\dots\dots (59),$$

$$\gamma = -\frac{1}{4} \frac{\omega^2 \rho \eta (a^2 + b^2)}{E} z \left( 1 - \frac{1}{3} \frac{z^2}{c^2} \right) \dots\dots\dots (60)$$

Denoting by  $2l$  the length of the long elliptic cylinder, we have

$$\delta c/c = -\frac{1}{8} (\omega^2 \rho \eta / E) (a^2 + b^2) = \frac{1}{8} (\delta l / l) \dots\dots\dots (61).$$

\* This enables the values of  $\delta c/c$ , for  $\eta = 0.25$  and  $b/a = 0, 0.2, 0.4, 0.6, 0.8$ , and 1, to be written down from Table XV, p. 159, of my paper on the elliptic cylinder. Table XVI of that paper gives values of  $\partial a/a$  and  $\partial b/b$  which apply unchanged to the long ellipsoid; while Tables XIII and XIV give its limiting angular velocity on the stress-difference and greatest strain theories.

The formula

$$\partial a/a - \partial b/b = \frac{\omega^2 \rho (1 + \eta) (a^2 - b^2) \{ (1 + \eta) (a^2 + b^2)^2 + a^4 + b^4 \}}{3 E (3a^4 + 2a^2b^2 + 3b^4)} \dots (62)$$

shows that of the principal transverse axes the longer is that which, even proportionately, is most extended.

\* 'Phil. Mag.,' Aug., 1892, formulæ (80) to (83), p. 156.

*Elongated Ellipsoid Rotating about a Short Axis 2a.*

§ 12. We have to substitute in equations (46) to (49)

$$P = 0, \quad Q = R = \omega^2.$$

Putting for shortness

$$4(1-\eta^2)(3a^4+2a^2b^2+3b^4) = \Pi'' \dots\dots\dots (63),$$

we get

$$\begin{aligned} \widehat{xx} = -\frac{\omega^2 \rho a^2}{\Pi''} & \left[ \{ (1-\eta^2)(a^4+3b^4) + (1-3\eta-6\eta^2)a^2b^2 \} \left( 1 - \frac{x^2}{a^2} \right) \right. \\ & - \{ (1-\eta^2)(4a^4+3a^2b^2+6b^4) - \eta(3+5\eta)a^2b^2 \} \frac{z^2}{c^2} \\ & \left. - \{ (1-\eta^2)(a^2+6b^2) - 3\eta(3+5\eta)a^2 \} y^2 \right] \dots\dots (64), \end{aligned}$$

$$\begin{aligned} \widehat{yy} = \frac{\omega^2 \rho b^2}{\Pi''} & \left[ \{ 5a^4+3a^2b^2+3b^4+3\eta a^2b^2 - \eta^2(5a^4-2a^2b^2+3b^4) \} \left( 1 - \frac{y^2}{b^2} \right) \right. \\ & \left. - a^2 \{ (1+3\eta+4\eta^2)b^2 + 2(1-\eta^2)a^2 \} \left( 3\frac{x^2}{a^2} + \frac{z^2}{c^2} \right) \right] \dots\dots (65), \end{aligned}$$

$$\begin{aligned} \widehat{zz} = \frac{\omega^2 \rho c^2}{\Pi''} & \left[ - \{ 3a^4+2a^2b^2+3b^4 + \eta(5a^4+9a^2b^2+6b^4) - \eta^2(3a^4-a^2b^2-6b^4) \right. \\ & - \eta^2(a^2-b^2)(5a^2+9b^2) \} \frac{x^2}{c^2} \\ & - \{ 3a^4+2a^2b^2+3b^4 + \eta(10a^4+a^2b^2+9b^4) + \eta^2(6a^4+a^2b^2-3b^4) \\ & \left. + \eta^3(a^2-b^2)(5a^2+9b^2) \} \frac{y^2}{c^2} \right. \\ & \left. + \{ \frac{1}{4}\Pi'' + \text{terms of order } a^2/c^2 \text{ and } b^2/c^2 \} \left( 1 - \frac{z^2}{c^2} \right) \right] \dots\dots (66), \end{aligned}$$

$$\widehat{yz} = -\frac{1}{4}\omega^2 \rho yz \dots\dots\dots (67),$$

$$\widehat{zx} = -\frac{1}{4}\omega^2 \rho zx \dots\dots\dots (68),$$

$$\widehat{xy} = \frac{\omega^2 \rho x y}{\Pi''} \{ (1-\eta^2)(a^4-3b^4) + 2\eta(3+5\eta)a^2b^2 \} \dots\dots\dots (69).$$

First approximations to the values of the stresses, strains, displacements, and increments of the semi-axes may be obtained by writing  $\omega^2$  for R in (51).

To this degree of approximation it makes no difference which of the two short axes is that about which rotation takes place. The two short axes shorten to the same extent per unit of length, while the long axis lengthens.

By writing  $\omega^2$  for Q and R in (54), we get a very close approximation to the strain  $s_x$  parallel to the long axis.

*Application of Method of Mean Values.*

§ 13. Let  $t_1, t_2, t_3$  be the mean lengths of material lines parallel to the principal axes  $2a, 2b, 2c$ , and let  $v$  be the volume of the ellipsoid. Then it is easily proved from the results in my paper\* on the mean values of strains, &c., that the elastic increments in the general case are given by

$$\delta t_1/t_1 = \frac{1}{5}(\rho/E)\{Pa^2 - \eta(Qb^2 + Rc^2)\} \dots\dots\dots (70),$$

$$\delta v/v = \frac{1}{15}(\rho/k)(Pa^2 + Qb^2 + Rc^2) \dots\dots\dots (71),$$

$$\delta t_1/t_1 + \delta t_2/t_2 + \delta t_3/t_3 = \delta v/v \dots\dots\dots (72).$$

In (71)  $k$  denotes the "bulk modulus."

For the gravitating nearly spherical ellipsoid, (70) gives

$$\delta t_1/t_1 = -\frac{4}{15}\pi\rho\rho^2a^2\frac{1-2\eta}{E}\left\{1 - \frac{1}{5}\frac{1-4\eta}{1-2\eta}\frac{a^2-b^2-c^2}{a^2}\right\} \dots\dots\dots (73).$$

Comparing (73) and (19) we see that in a gravitating perfect sphere

$$\delta t_1/t_1 = \delta a/a,$$

or the reductions per unit length of a diameter and of the mean parallel chord are identical.

For the increment in volume in the gravitating nearly spherical ellipsoid, (71) gives

$$\delta v/v = -4\pi\rho\rho^2(a^2 + b^2 + c^2)/(45k) \dots\dots\dots (74). \dagger$$

For the gravitating very flat spheroid of § 7, we find

$$\left. \begin{aligned} \delta t_1/t_1 &= -\pi^2\rho\rho^2ac(1-\eta)/5E, \\ \delta t_2/t_2 &= 2\pi^2\rho\rho^2ac\eta/5E, \\ \delta v/v &= -2\pi^2\rho\rho^2ac(1-2\eta)/5E \end{aligned} \right\} \dots\dots\dots (75).$$

For the general case of an ellipsoid rotating about  $2a$ , we have

$$\left. \begin{aligned} \delta t_1/t_1 &= -\frac{1}{5}\frac{\omega^2\rho\eta}{E}(b^2 + c^2), \\ \delta t_2/t_2 &= \frac{1}{5}\frac{\omega^2\rho}{E}(b^2 - \eta c^2), \\ \delta v/v &= \omega^2\rho(b^2 + c^2)/(15k) \end{aligned} \right\} \dots\dots\dots (76).$$

\* 'Camb. Phil. Soc. Trans.,' vol. 15, pp. 313-337.

† Agrees with formula (105), p. 335, 'Camb. Trans.,' *loc. cit.*

A material line parallel to an axis  $2b$ , perpendicular to the axis of rotation, is exposed to two opposing actions. The components of "centrifugal" force in its own direction tend to lengthen it, while those in the perpendicular direction  $2c$  tend to shorten it. On an average the former action will prevail so long as the mean dimension parallel to  $2b$  bears to that parallel to  $2c$  a ratio exceeding  $\sqrt{\eta} : 1$ .

*Approximate Methods.*

§ 14. Suppose a body symmetrical with respect to the coordinate planes, of great length,  $2c$ , in the direction of  $z$ , to be acted on by the bodily forces whose components are  $Px$ ,  $Qy$ ,  $Rz$ . The stresses over the plane  $z = z$  must balance the force whose components are

$$\iiint \rho Px \, dx \, dy \, dz, \text{ \&c.}$$

Since every section perpendicular to  $z$  has its c.g. on that axis, it is clear the integrals vanish, which give the components parallel to  $x$  and  $y$ . If now we assume that when there is no rapid variation in the cross section,  $\sigma$ , as  $z$  alters,  $\widehat{zz}$  is large compared to the other stresses, and is approximately uniform over  $\sigma$ , then we may take as a first approximation

$$\widehat{zz} = \frac{R\rho}{\sigma} \int_z^c \iint z \, dz \, dy \, dx.$$

For an elongated ellipsoid this gives, as in (51),

$$\widehat{zz} = \frac{1}{4} R\rho (c^2 - z^2).$$

Taking this as the sole stress, we obtain from the ordinary stress-strain relations the same values of  $s_x$ ,  $s_y$ ,  $s_z$  as in (51), and thence by integration the correct first approximation values of the displacements.

For a thin elliptic disc in which one of the axes,  $2c$ , of the elliptic section is very large compared to the other or to the thickness, we find as first approximations

$$\left. \begin{aligned} \widehat{zz} &= \frac{1}{3} R\rho (c^2 - z^2), \\ \alpha/x = \beta/y &= -\frac{1}{3} R\rho \eta (c^2 - z^2)/E, \\ \gamma &= \frac{1}{3} R\rho z (c^2 - \frac{1}{3} z^2)/E \end{aligned} \right\} \dots\dots\dots (77).$$

If, for instance, the disc be rotating about its thickness, *i.e.*, the axis of the cylinder, then  $R = \omega^2$  and

$$\left. \begin{aligned} \alpha/x = \beta/y &= -\frac{1}{3} \omega^2 \rho \eta (c^2 - z^2)/E, \\ \gamma &= \frac{1}{3} \omega^2 \rho z (c^2 - \frac{1}{3} z^2)/E \end{aligned} \right\} \dots\dots\dots (78).$$

As a third example, suppose the section perpendicular to the long axis uniform in shape and area, we then get

$$\left. \begin{aligned} \bar{z} &= \frac{1}{2} R \rho (c^2 - z^2), \\ \alpha/x = \beta/y &= -\frac{1}{2} R \rho \eta (c^2 - z^2)/E, \\ \gamma &= \frac{1}{2} R \rho z (c^2 - \frac{1}{2} z^2)/E \end{aligned} \right\} \dots\dots\dots (79).$$

A special instance of this last case is presented by an elongated cylinder rotating about a perpendicular to its length through its centre. Putting  $R = \omega^2$ , we have

$$\left. \begin{aligned} \alpha/x = \beta/y &= -\frac{1}{2} \omega^2 \rho \eta (c^2 - z^2)/E, \\ \gamma &= \frac{1}{2} \omega^2 \rho z (c^2 - \frac{1}{2} z^2)/E \end{aligned} \right\} \dots\dots\dots (80).*$$

Comparing the several cases of rotation, we have an interesting illustration of how the effects of the "centrifugal" force increase as the mean distance of the substance of the solid from the axis of rotation becomes larger. If the limiting angular velocity permissible in the elongated ellipsoid rotating about a short axis be taken as 100, then the limiting angular velocities in the thin elliptic disc and the elongated cylinder rotating about their short axes—the material, and the length of the long dimension being the same for all—are approximately 87 and 71 respectively, both on the stress-difference and greatest strain theories. A caution must, however, be added that in bodies of such elongated form rotating about a short axis, a sudden change in the angular velocity may prove disastrous.

"Micro-Metallography of Iron. Part I." By THOMAS ANDREWS, F.R.S., M.Inst.C.E. Received December 15, 1894,—Read January 24, 1895.

*Secondary Micro-crystalline Structure in Metallic Iron.*

The term metallography appositely describes, in one word, that department of metallurgical science which deals with the accurate study and delineation of the ultimate formation or structure of metals, a knowledge of which is of the utmost importance.

This development of the science of metallurgy is destined to prove of incalculable value in the study of the ultimate micro-structure of iron and steel.

Experiments on the microscopic structure of iron and steel appear

\* This is more exact for a long beam of rectangular cross section than the result I obtained in the 'Quarterly Journal' for 1888, p. 29.

to have been first initiated by Dr. H. Clifton Sorby, F.R.S., about thirty years ago, and the author about the same time commenced some investigations in this direction. Strange to say, the subject has since then lain almost dormant, and only recently have scientific investigators resumed the study of the ultimate microscopic structure of metals. Comparatively little progress has yet been made in this important and fertile field of research, and our knowledge of the true structure of metals is, consequently, at the present time limited. Excellent work in this direction is now, however, in progress at the hands of several investigators, and it is hoped that by means of further careful detailed observation with accurate modern microscopic appliances valuable and reliable results will accrue.

In the course of a research with high microscopical powers (including 300, 500, 800, 1200, and upwards to 2000 diameters) on the micro-crystalline structure of large masses of wrought iron, the author had the privilege recently to observe the following novel metallurgical facts.

When large masses, several tons in weight, of practically pure wrought iron were allowed to slowly cool from a white heat, a secondary or subcrystallisation of the metallic iron occurred. The normal primary crystals of the iron, or those which have hitherto been regarded as constituting the ultimate structure of the metal, were found to inclose a subcrystalline formation consisting of very minute, and much smaller, crystals of pure iron also belonging to the regular order of crystallisation. These crystals sometimes manifested the hexagonal form, the predominant angle being about  $120^\circ$ , and often they assumed the form of simple cubes. The secondary crystals were contained within the area of the larger primary crystals.

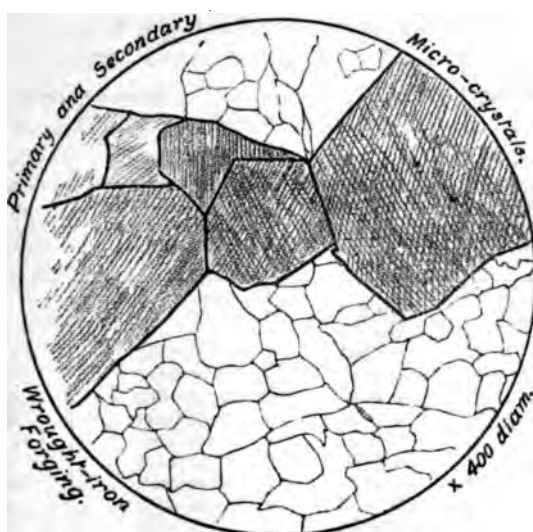
Typical illustrations of this duplex crystallisation found in two large iron forgings are given in figs. 1 and 2, and the relative dimensions of a number of individual crystals are given on Tables I and II.

The results of twenty measurements of the primary crystals and twenty measurements of the secondary crystals taken on each forging are given on these tables.

The markings of the intercrystalline spaces or junctions of the secondary crystals were very clearly defined, but they were exceedingly minute. The general form, contour, and relative size of the primary and secondary crystals, as seen in section, will be noticed on reference to the accurate tracings, figs. 1 and 2.

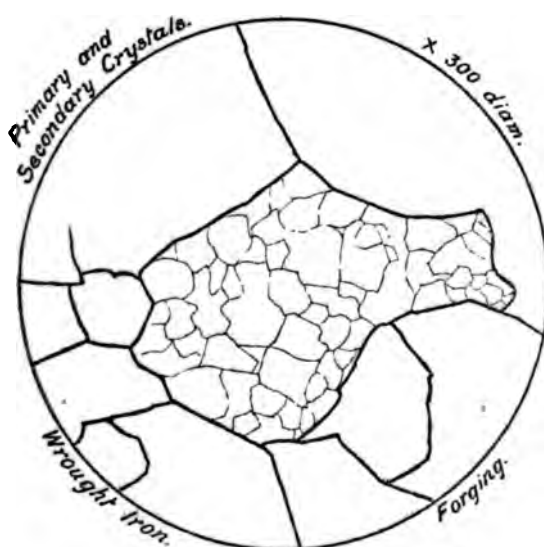
Judging roughly from the indications of the average micro-measurements on Tables I and II, there would appear to be approximately 1,000,000,000 of the secondary crystals in a cubic inch of the metallic iron.

FIG. 1.



Micro-section from large wrought-iron forging, showing primary and secondary micro-crystalline formation.

FIG. 2.



Micro-section from large wrought-iron forging, showing primary and secondary micro-crystalline formation.

Table I.—Secondary Micro-crystallisation of Metallic Iron.

| Micro-section from large iron forging No. 1.                        |                                                                   |                                                                     |                                                                   |
|---------------------------------------------------------------------|-------------------------------------------------------------------|---------------------------------------------------------------------|-------------------------------------------------------------------|
| Primary crystals.                                                   |                                                                   | Secondary crystals.                                                 |                                                                   |
| Longitudinal dimensions of individual crystals in parts of an inch. | Transverse dimensions of individual crystals in parts of an inch. | Longitudinal dimensions of individual crystals in parts of an inch. | Transverse dimensions of individual crystals in parts of an inch. |
| 0·0273                                                              | 0·0182                                                            |                                                                     |                                                                   |
| 0·0091                                                              | 0·0059                                                            |                                                                     |                                                                   |
| 0·0091                                                              | 0·0068                                                            | 0·0012                                                              | 0·0008                                                            |
| 0·0187                                                              | 0·0114                                                            | 0·0014                                                              | 0·0008                                                            |
| 0·0050                                                              | 0·0045                                                            | 0·0016                                                              | 0·0016                                                            |
| 0·0032                                                              | 0·0027                                                            | 0·0008                                                              | 0·0006                                                            |
| 0·0077                                                              | 0·0073                                                            | 0·0010                                                              | 0·0008                                                            |
| 0·0159                                                              | 0·0136                                                            | 0·0018                                                              | 0·0016                                                            |
| 0·0227                                                              | 0·0159                                                            | 0·0012                                                              | 0·0008                                                            |
| 0·0136                                                              | 0·0136                                                            | 0·0012                                                              | 0·0010                                                            |
| 0·0136                                                              | 0·0114                                                            | 0·0010                                                              | 0·0008                                                            |
| 0·0109                                                              | 0·0109                                                            | 0·0008                                                              | 0·0006                                                            |
| 0·0068                                                              | 0·0050                                                            | 0·0020                                                              | 0·0020                                                            |
| 0·0182                                                              | 0·0159                                                            | 0·0006                                                              | 0·0006                                                            |
| 0·0054                                                              | 0·0045                                                            | 0·0016                                                              | 0·0012                                                            |
| 0·0045                                                              | 0·0045                                                            | 0·0008                                                              | 0·0006                                                            |
| 0·0045                                                              | 0·0045                                                            | 0·0020                                                              | 0·0020                                                            |
| 0·0091                                                              | 0·0068                                                            | 0·0012                                                              | 0·0010                                                            |
| 0·0118                                                              | 0·0114                                                            | 0·0008                                                              | 0·0006                                                            |
| 0·0068                                                              | 0·0054                                                            | 0·0008                                                              | 0·0006                                                            |
| Average<br>0·0112                                                   | Average<br>0·0090                                                 | Average<br>0·0012                                                   | Average<br>0·0010                                                 |

In the case of both the primary and secondary crystals the predominant well-defined angles of the facets of the crystals hovered more or less about the angle of  $120^{\circ}$ . The majority of the angle readings, made with the goniometer attached to the microscope, indicated generally a hexagonal structure or form of crystallisation. There were, however, also perfect cubical crystals observed.

The observations were made with a Ross' first-class microscope. The micro-measurements afford an indication of the comparative size of the primary and secondary crystals. These measurements were carefully taken by a Jackson micrometer and in some cases by a Ramsden screw micrometer, both accurately calibrated with a standard stage micrometer. The wrought-iron forgings on which the observations were made were constituted of practically pure hammered wrought iron, the dimensions of the mass being about 10 feet long and about 12 inches square. The great length of time required for

such large masses of iron to cool from a white heat appeared to facilitate the production of the crystals of the secondary formation.

Table II.—Secondary Micro-crystallisation of Metallic Iron.

| Micro-section from large iron forging No. 2.                        |                                                                   |                                                                     |                                                                   |
|---------------------------------------------------------------------|-------------------------------------------------------------------|---------------------------------------------------------------------|-------------------------------------------------------------------|
| Primary crystals.                                                   |                                                                   | Secondary crystals.                                                 |                                                                   |
| Longitudinal dimensions of individual crystals in parts of an inch. | Transverse dimensions of individual crystals in parts of an inch. | Longitudinal dimensions of individual crystals in parts of an inch. | Transverse dimensions of individual crystals in parts of an inch. |
| 0·0140                                                              | 0·0100                                                            | 0·00100                                                             | 0·00066                                                           |
| 0·0060                                                              | 0·0050                                                            | 0·00050                                                             | 0·00024                                                           |
| 0·0060                                                              | 0·0030                                                            | 0·00050                                                             | 0·00050                                                           |
| 0·0180                                                              | 0·0120                                                            | 0·00066                                                             | 0·00050                                                           |
| 0·0160                                                              | 0·0080                                                            | 0·00100                                                             | 0·00083                                                           |
| 0·0090                                                              | 0·0048                                                            | 0·00083                                                             | 0·00083                                                           |
| 0·0060                                                              | 0·0052                                                            | 0·00050                                                             | 0·00050                                                           |
| 0·0084                                                              | 0·0060                                                            | 0·00033                                                             | 0·00033                                                           |
| 0·0060                                                              | 0·0030                                                            | 0·0022                                                              | 0·0016                                                            |
| 0·0090                                                              | 0·0070                                                            | 0·0010                                                              | 0·0008                                                            |
| 0·0100                                                              | 0·0060                                                            | 0·0016                                                              | 0·0012                                                            |
| 0·0050                                                              | 0·0044                                                            | 0·0010                                                              | 0·0008                                                            |
| 0·0050                                                              | 0·0040                                                            | 0·0010                                                              | 0·0008                                                            |
| 0·0100                                                              | 0·0050                                                            | 0·0004                                                              | 0·0004                                                            |
| 0·0048                                                              | 0·0046                                                            | 0·0012                                                              | 0·0012                                                            |
| 0·0072                                                              | 0·0054                                                            | 0·0008                                                              | 0·0008                                                            |
| 0·0080                                                              | 0·0060                                                            | 0·0010                                                              | 0·0008                                                            |
| 0·0056                                                              | 0·0016                                                            | 0·0010                                                              | 0·0008                                                            |
| 0·0110                                                              | 0·0070                                                            | 0·0010                                                              | 0·0008                                                            |
| 0·0080                                                              | 0·0060                                                            | 0·0008                                                              | 0·0006                                                            |
| Average<br>0·0086                                                   | Average<br>0·0057                                                 | Average<br>0·00097                                                  | Average<br>0·00075                                                |

In the case of both the primary and secondary crystals the predominant well-defined angles of the facets of the crystals hovered more or less about the angle of  $120^{\circ}$ . The majority of the angle readings, made with the goniometer attached to the microscope, indicated generally a hexagonal structure or form of crystallisation. There were, however, also perfect cubical crystals observed.

The *rationale* of this duplex crystallisation has apparently been as follows:—The mass of metallic iron on cooling having reached the crystallising point at about  $740^{\circ}$  C., the periphery or skeletons of the larger or primary crystals were then formed. As the period of cooling was, however, very slow, the semi-fluid or viscous metal in the interior of these primary crystals was, on finally consolidating,

apparently further broken up or subdivided into a considerable number of smaller crystals, inclosed within the boundary or periphery of the primary crystals.

In the course of further experiments on the cooling of large masses of wrought iron, the author has also found, by the use of high power objectives, that the secondary crystals sometimes inclosed a still more minute form of crystals of pure iron, of the cubical form, which may hence be regarded as constituting a tertiary system of crystallisation in pure metallic iron. These experiments therefore indicate that large masses of heated wrought iron, on cooling from above the temperature of the crystallisation of metallic iron, viz.,  $740^{\circ}\text{C}.$ ,\* are capable of crystallising in three distinct modifications, which may tentatively be called the primary, secondary, and tertiary system of crystallisation in iron, these various crystalline modifications being all, however, connected with the regular system of crystallisation. The author has microscopically examined numerous large masses of practically pure wrought iron varying in weight from about 2 tons to 4 cwt., and even less, and he finds these subcrystalline formations to be frequently present, consequent on the slow cooling of such large masses.

The crystals of this secondary formation are not often distinctly discernible in smaller masses of metallic iron, such as rolled rods, plates, or sheets, as these in the course of manufacture rapidly cool, and are frequently manipulated during the finishing processes at temperatures below the crystallising point of wrought iron ( $740^{\circ}\text{C}.$ ). The author has, however, observed the presence of this subcrystalline structure in small masses of iron, but, in these instances, the subcrystals are generally smaller in size and not always so distinctly marked as those found in larger masses of metallic iron.

The microscopical examinations were made on carefully-prepared and polished samples, etched in nitric acid (1 part  $\text{HNO}_3$ , sp. gr. 1.20, and 49 parts water), and by the use of high microscopical powers ( $\frac{1}{8}$  inch to  $\frac{1}{16}$  inch, and other objectives). The drawings were accurately made with the camera Lucida.

In each observation the etching was prolonged, under constant observation with lenses, a suitable time to develop the accurate structure of the metal.

The varied forms of crystallisation observed in this research and referred to in this paper appear to be of such novel metallurgical interest, that the author felt it desirable at once to record the observations, and he hopes to be able to furnish the results of further investigations in this direction.

\* The temperature at which pure iron crystallises, viz.,  $740^{\circ}\text{C}.$ , has recently been approximately determined with great care and accuracy by Professor J. O. Arnold, F.O.S., at the Sheffield Technical School.

*April 25, 1895.*

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On a Gas showing the Spectrum of Helium, the reputed cause of  $D_3$ , one of the Lines in the Coronal Spectrum. Preliminary Note." By WILLIAM RAMSAY, F.R.S., Professor of Chemistry, University College, London. Received March 26, 1895.

In the course of investigations on argon, some clue was sought for, which would lead to the selection of one out of the almost innumerable compounds with which chemists are acquainted, with which to attempt to induce argon to combine. A paper by W. F. Hillebrand, "On the Occurrence of Nitrogen in Uraninite, &c." ('Bull. of the U.S. Geological Survey,' No. 78, p. 43), to which Mr. Miers kindly directed my attention, gave the desired clue. In spite of Hillebrand's positive proof that the gas he obtained by boiling various samples of uraninite with weak sulphuric acid was nitrogen (p. 55)—such as formation of ammonia on sparking with hydrogen, analysis of the platinichloride, vacuum-tube spectrum, &c.—I was sceptical enough to doubt that any compound of nitrogen, when boiled with acid, would yield free nitrogen. The result has justified the scepticism.

The mineral employed was clèveite, essentially a uranate of lead, containing rare earths. On boiling with weak sulphuric acid, a considerable quantity of gas was evolved. It was sparked with oxygen over soda, so as to free it from nitrogen and all known gaseous bodies except argon; there was but little contraction; the nitrogen removed may well have been introduced from air during this preliminary experiment. The gas was transferred over mercury, and the oxygen absorbed by potassium pyrogallate; the gas was removed, washed with a trace of boiled water, and dried by admitting a little sulphuric acid into the tube containing it, which stood over mercury. The total amount was some 20 c.c.

Several vacuum-tubes were filled with this gas, and the spectrum was examined, the spectrum of argon being thrown simultaneously into the spectroscope. It was at once evident that a new gas was present along with argon.

Fortunately, the argon-tube was one which had been made to try

whether magnesium-poles would free the argon from all traces of nitrogen. This it did; but hydrogen was evolved from the magnesium, so that its spectrum was distinctly visible. Moreover, magnesium usually contains sodium, and the D line was also visible, though faintly, in the argon-tube. The gas from clèveite also showed hydrogen lines dimly, probably through not having been filled with completely dried gas.

On comparing the two spectra, I noticed at once that while the hydrogen and argon lines in both tubes accurately coincided, a brilliant line in the yellow, in the clèveite gas, was nearly *but not quite* coincident with the sodium line D of the argon-tube.

Mr. Crookes was so kind as to measure the wave-length of this remarkably brilliant yellow line. It is 587.49 millionths of a millimetre, and is exactly coincident with the line D<sub>2</sub> in the solar chromosphere, attributed to the solar element which has been named *helium*.

Mr. Crookes has kindly consented to make accurate measurements of the position of the lines in this spectrum, which he will publish, and I have placed at his disposal tubes containing the gas. I shall therefore here give only a general account of the appearance of the spectrum.

While the light emitted from a Pfücker's tube charged with argon is bright crimson, when a strong current is passed through it, the light from the helium-tube is brilliant golden yellow. With a feeble current the argon-tube shows a blue-violet light, the helium-tube a steely blue, and the yellow line is barely visible in the spectroscope. It appears to require a high temperature therefore to cause it to appear with full brilliancy, and it may be supposed to be part of the high-temperature spectrum of helium.

The following table gives a qualitative comparison of the spectra in the argon\* and in the helium-tubes.

| Argon-tube.     |                   | Helium-tube.     |                     |
|-----------------|-------------------|------------------|---------------------|
| Red .....       | { 1st triplet.    | { 1st triplet.   | Equal in intensity. |
|                 | { 2nd pair.       | { 2nd pair.      | " "                 |
|                 | { Faint line.     | { Faint line.    | " "                 |
|                 | { Stronger line.  | { Stronger line. | " "                 |
|                 | { Brilliant line. | { Dull line.     | } Weak in helium.   |
|                 | { Strong line.    | { Very dim line. |                     |
| Red-orange ...  | { Moderate Line.  | { Moderate line. | Equal in intensity. |
|                 | { " "             | { " "            | " "                 |
|                 | { " "             | { " "            | " "                 |
| Orange .....    | { Faint line.     | { Faint line.    | " "                 |
|                 | { Triplet.        | { Triplet.       | " "                 |
| Orange-yellow . | Pair.             | Pair.            | " "                 |

\* The tube then used was the one with which Mr. Crookes's measurements of the argon spectrum were made. It contains absolutely pure atmospheric argon.

| Argon-tube.      |                   | Helium-tube.           |                                                   |
|------------------|-------------------|------------------------|---------------------------------------------------|
| Yellow .....     | Absent.           | Brilliant.             | W = 587·49.<br>(the helium line, D <sub>3</sub> ) |
| Green .....      | 7 lines.          | 7 lines.               | Equal in intensity.                               |
| Green-blue ....  | 5 lines.          | 5 lines.               | In helium only.                                   |
|                  | Absent.           | Faint.                 |                                                   |
| Blue .....       | Absent.           | Brilliant.             | " "                                               |
|                  | Absent.           | 8 lines.               | " "                                               |
| Blue-violet .... | 3 lines, strong.  | Barely visible, if in- | Equal in intensity.                               |
|                  | 2, fairly strong. | deed present at all.   |                                                   |
| Violet .....     | Absent.           | Bright line.           | In helium only.                                   |
|                  | Absent.           | 4 bright lines.        |                                                   |
|                  | Violet pair.      | Violet pair.           | Equal in intensity.                               |
|                  | Single line.      | Single line.           | " "                                               |
|                  | Triplet.          | Triplet.               | " "                                               |
|                  | Triplet.          | Triplet.               | " "                                               |
|                  | Pair.             | Pair.                  | " "                                               |

It is to be noticed that argon is present in the helium-tube, and by the use of two coils the spectra could be made of equal intensity. But there are sixteen easily visible lines present in the helium-tube only, of which one is the magnificent yellow, and there are two red lines strong in argon and three violet lines strong in argon, but barely visible and doubtful in the helium-tube. This would imply that atmospheric argon contains a gas absent from the argon in the helium-tube. It may be that this gas is the cause of the high density of argon, which would place its atomic weight higher than that of potassium.

It is idle to speculate on the properties of helium at such an early stage in the investigation; but I am now preparing fairly large quantities of the mixture, and hope to be able before long to give data respecting the density of the mixture, and to attempt the separation of argon from helium.

(Note added June 14.—It is now practically certain that the presence of so many of the argon lines in the helium spectrum must have been due to the accidental introduction of air. But there still are coincidences, chiefly in the red lines, which would justify the supposition that there is some constituent common to the two gases.)

## II. "On the new Gas obtained from Uraninite. Preliminary Note." By J. NORMAN LOCKYER, C.B., F.R.S. Received April 25, 1895.

On the 28th of March, Professor Ramsay was so good as to send me a tube containing a new gas obtained by him from uraninite (clèveite), showing a line in the yellow which was stated to be of the

same wave-length as  $D_2$ , which I discovered in 1868. This line Dr. Frankland and myself shortly afterwards suggested might be a line of hydrogen, not visible under laboratory conditions; but solar work subsequently showed that this view was untenable, although the gas which produced it was certainly associated with hydrogen.

Subsequently, other chromospheric lines were found to vary with the yellow line, and the hypothetical gas which gave rise to them was provisionally named "helium," to differentiate it from hydrogen.

It was therefore of great interest to me to learn whether the new gas was veritably that which was responsible for the solar phenomena in question, and I am anxious to express my best thanks to Professor Ramsay for sending the tube to enable me to form an opinion on this matter. Unfortunately, it had been used before I received it, and the glass was so blackened that the light was invisible in a spectro-scope of sufficient dispersion to decide the question.

On March 29th therefore, as Professor Ramsay was absent from England, in order not to lose time, I determined to see whether the gas which had been obtained by chemical processes would not come over by heating *in vacuo*, after the manner described by me to the Society in 1879,\* and Mr. L. Fletcher was kind enough to give me some particles of uraninite (bröggerite) to enable me to make the experiment.

This I did on March 30th, and it succeeded; the gas giving the yellow line came over, associated with hydrogen, in good quantity.

I have since obtained photographs of the spectrum of the gas, both in vacuum tubes while the Sprengel pump has been going, and at atmospheric pressure over mercury. To-day I limit myself to exhibiting two of these photographs.

One of the photographs exhibits a series of spectra taken during the action of the pump. The two lower spectra indicate the introduction of air by a leak after the capillary had cracked near one of the platitudes, giving us on the same plate the banded and line spectrum of air. These prove that there was no air present in the tube when the fourth spectrum was taken. This photograph has not yet been finally reduced, but a preliminary examination has indicated that most of the lines are due to the structure spectrum of hydrogen, but not all of them.

Among the lines which cannot be referred to this origin are two respectively near  $\lambda$  4471 and  $\lambda$  4302, which have been observed in the chromosphere, 4471 being as important as  $D_2$  itself, from the theoretical point of view, to students of Solar Physics.

Whilst spectrum No 4 was being photographed with the capillary tube end-on-wise, eye observations were made in another spectro-scope directed sideways at it. I give from the Laboratory Note Book the

\* 'Roy. Soc. Proc.,' vol. 29, p. 266.

observations I made while photograph No. 4 was being taken, to show that the yellow line was visible during the whole exposure.

Thursday, April 4th, 1895.

Plate F. Exposure 4.

- Ten minutes' exposure.

- 4.42. Exposure started.
  - 4.43. Yellow line brightening up considerably.
  - 4.44. Suddenly as bright as hydrogen.
  - 4.45. Yellow line double.
  - 4.46. Comparison with D gives yellow line in position of D.
  - 4.47. Pump much less full; 7 c.c. of gas collected. Yellow line much brighter.
  - 4.48. Air-break introduced. Line still visible, but very faint. Hydrogen lines getting brighter and some double lines appearing in green.
  - 4.48.5. Air-break and jar removed. Yellow line the only one seen, being as bright as C. Line in green the only other line visible.
  - 4.50. Replaced jar. Yellow brightening and the other lines more refrangible, brightening with it.
  - 4.51. Very bright. Stopcock nearly full of gas.
  - 4.52. End of exposure.

The lines which appear best in the photographs of the capillary tube and of the gas collected over mercury, are as follows. The lines indicated by an asterisk are those lines recorded in the chromosphere by Young or myself, or photographed during the eclipses of 1896.

| Micrometer reading. | Wavelength (Angstroms). |                   |
|---------------------|-------------------------|-------------------|
| 3.2495              | 4870*                   | much              |
| .2917               | 4870*                   | is alto-          |
| .2981               | 4870*                   | aturated          |
| .3234               | 4870*                   | n, and is         |
| .3316               | 4870*                   | reagents.         |
| .4146               | 4870*                   | and subse-        |
| .5740               | 4870*                   | ant is about      |
| .5884               | 4870*                   | it gives the      |
| .5933               | 4870*                   |                   |
| .6139               | 4870*                   | bobbin of the     |
| .6176               | 4870*                   | ried at 100° C.,  |
| .6262               | 4870*                   | which corresponds |
| .6290               | 4870*                   |                   |

With regard to the observations made on the gas obtained from uraninite, I found the uraninite gas, as obtained from uraninite, to be an amorphous gas, as obtained from uraninite, used in North America.

contain the argon lines as given by Mr. Crookes; nor, with the exception of the yellow line, do I get the special lines noted by him in the gas. (Four of these out of six seem possibly to be due to nitrogen.)

But I do get lines nearly coinciding with chromospheric lines discovered by me in 1868.

On November the 6th of that year, I suspected a line less refrangible than C, and so near it that when both were showing brilliantly the pair appeared double, like D in a spectroscope of moderate dispersive power.\*

Later, I discovered another line at 6678.3 (Rowland), which was observed to vary with D<sub>3</sub>. There is a line in this position, with the dispersion employed, in the spectrum of the new gas. This line has also been seen by Thalèn, as stated by Professor Cleve in a communication to the Paris Academy ('Comptes Rendus,' April 16th, p. 835), but the other lines given by him (with the possible exception of the one at 5016) have not been recorded by me.

Although I have, at present, been unable to make final comparisons with the chromospheric lines, the evidence so far obtained certainly lends great weight to the conclusion that the new gas is one effective in producing some of them, and it is suggested by the photographs that the structure lines of hydrogen may also be responsible for some of them.

I may state, under reserve, that I have already obtained evidence that the method I have indicated may ultimately provide us with other new gases, the lines of which are also associated with those of the chromosphere.

Messrs. Fowler, Baxandall, Shackleton, and Butler have assisted me in the various stages of the inquiry.

III. "*Acokanthera Schimperi*: its Natural History, Chemistry, and Pharmacology." By THOMAS R. FRASER, M.D., LL.D., F.R.S., Professor of Materia Medica in the University of Edinburgh; and JOSEPH TILLIE, M.D., F.R.S.E., Lecturer on Experimental Pharmacology in the University of Edinburgh. Received March 28, 1895.

(Abstract.)

Several years ago an opportunity was given to one of us to examine poisoned arrows and the poison used in smearing them, of the Wa Nyika tribe of East Africa. While the pharmacological action of this poison was found to have a close resemblance to that

\* 'Phil. Trans.,' 1869, p. 428.

of *Strophanthus* seeds, its physical and chemical properties enabled the conclusions to be drawn that the poison was not made from these seeds, but was chiefly composed of an extract prepared from a wood. These conclusions have been confirmed by the examination of further specimens of the Wa Nyika arrow-poison, and of the wood from which it is prepared.

Specimens of the Wa Gyriama and of the Wa Kamba arrow-poisons and of the wood from which they are prepared have also been examined.

We have been enabled to refer all these specimens of wood to the genus *Acokanthera*, which, therefore, supplies the arrow-poison used over an extensive area in East Africa.

Leaves, flowers, and fruit, each taken from the same individual tree, having also been sent to us, we have been enabled to determine that the wood of the species *Acokanthera Schimperi*, Benth. and Hook. (*Carissa Schimperi*, A.DC.), is used by the Wa Nyika and the Wa Gyriama and other tribes in preparing their arrow-poison.

The tribes inhabiting the coast regions near Mombasa have long been known to use an arrow-poison, but the botanical specimens received by us are apparently the first that have been completely identified.

The arrow-poison of these tribes usually contains a crystalline glucosidal active principle, which, in its chemical properties and pharmacological action, is identical with the active principle also separated by us from the wood of *Acokanthera Schimperi*.

This active principle crystallises from water in the form of colourless, transparent, quadrangular plates, and from alcohol in colourless, thin, needle-shaped crystals, which usually group themselves in tufts and rosettes. At a temperature of 13—15° C. it is soluble to the extent of 0.93 per cent. in distilled water, and of 2.4 per cent. in diluted alcohol of sp. gr. 0.920. At higher temperatures much larger quantities are dissolved by water and by alcohol. It is altogether insoluble in ethylic ether and in chloroform. A saturated solution in cold water is tasteless and neutral in reaction, and is affected, in an obvious manner, by very few chemical reagents. Strong sulphuric acid produces immediately a red colour, and subsequently a green colour is developed. The melting point is about 186° C. When treated with dilute sulphuric acid it gives the reaction of a glucoside.

Concordant combustions made for us by Dr. Dobbin of the Chemical Laboratory of the University show that, dried at 100° C., it contains (ii) C 58.46 per cent., H 7.71 per cent., which corresponds with the formula  $C_{20}H_{18}O_{13}$ .

In 1882 MM. Rochebraune and Arnaud separated an amorphous glucoside from the root of an unknown species of tree used in North

Somaliland to prepare an arrow-poison. This glucoside, dried at 120° C., on combustion yielded C 48.3 per cent., H 6.5 per cent., and to it the name ouabaïn was given.

In 1888 Arnaud obtained from the wood of an unidentified species of *Acokanthera*, provisionally named *Acokanthera ouabaïo*, Cathelineau (*Carissu ouabaïo*, Franchet and Poisson), used in North Somaliland to prepare an arrow-poison, a crystalline glucosidal active principle, which, dried at 140° C., on combustion yielded (i) C 58.14 per cent., H 7.67 per cent., corresponding with the formula  $C_{20}H_{44}O_{13}$ , and dried at 100° C., with the formula  $C_{20}H_{44}O_{13}, H_2O$ ; and to it the name ouabaïn was also given.

In 1893 Lewin, and also Merck, separated from the wood of another *Acokanthera*, named *Acokanthera Deflersii*, Schweinfurth, an amorphous glucosidal active principle, which, dried at 100° C., on combustion yielded C 58.32 per cent., H 8.01 per cent.; and which is also named by Lewin ouabaïn, and erroneously stated to be the active principle of *Acokanthera Schimperi*.

The complete recognition of the species of *Acokanthera* is of primary importance, because several supplies of the wood of unidentified species of *Acokanthera* sent to us from East Equatorial Africa yielded only a glucosidal active principle, which was amorphous.

The characters of the crystalline active principle which we have separated from the wood of the fully identified species *Acokanthera Schimperi*, Benth. and Hook., agree with those of the crystalline active principle separated by Arnaud from the wood of the unidentified species of *Acokanthera*, provisionally named *ouabaïo*; and also from the seeds of an unidentified species of *Strophanthus*, obtained from West Africa.

As, therefore, the name ouabaïn is used for three quite different substances, and is itself derived from merely a vernacular word "wabayo," which is used in a restricted district, we would suggest that, in accordance with a usual custom, the crystalline active principle of *Acokanthera Schimperi* should be named acokantherin and not ouabaïn.

In the Preliminary Notice of this paper there was appended a note, making brief mention of some of the most important general observations made by several investigators upon the pharmacological action of those arrow-poisons which, at the time when the investigations were made, were not known to be derived from the *Acokanthera* species, and upon several non-crystalline and crystalline glucosidal substances to which the arrow-poisons owe their action. In the present paper the work accomplished by Arnott and by Haines in 1853, by Kinger in 1880, by Rochebraune and Arnaud in 1881, by Laborde in 1887, by Langlois and Varigny, by Gley and Rondeau,

and by Gley in 1888, by Sailer in 1891, by Paschkis in 1892, and by Lewin in 1893, has been more fully described.

The group of arrow-poisons which owes its activity to extracts of *Acokanthera* plants has been found to possess a qualitative identity of action with *Strophanthus*, but some of the authors mentioned emphasise an action upon the cardio-respiratory centres in the medulla, and others a direct action upon the heart.

A detailed examination of the pharmacological action of acokantherin has not led to the discovery of any important qualitative differences between its action and that of *Strophanthus hispidus* and of its active principle strophanthin, which was described by one of us in 1870, in 1872, and in 1890.

As, however a special interest must be attached to the effects upon the circulation, the experiments upon the heart, blood-vessels, and blood-pressure are described with more detail than those upon other systems. Our experiments show that small and carefully-regulated doses can produce a great slowing of the rate of the heart, even when the vagi nerves are divided, or when atropine is administered; and that a great increase in the extent of the diastolic and systolic movements of the heart can be obtained without the average blood-pressure being at all affected, or affected only to a very small extent. Any rise of blood-pressure which follows the administration of such doses is accompanied by so great a slowing of the rate and by so great an increase of the extent of the pulse movement that constriction of blood-vessels seems contra-indicated; and the rise of blood-pressure must therefore be attributed to the increase in the amplitude and vigour of the heart movements and the greater quantity of blood propelled into the arteries. Large doses produce a rise of blood-pressure, which is probably due to an action upon the vaso-motor centres or peripheral ganglia, and probably not upon the muscle of the blood-vessels, because perfusion experiments in frogs show that dilute solutions of acokantherin of 1 in 100,000 to 1 in 10,000 in normal saline do not produce contraction of blood-vessels, and with even so strong a solution of acokantherin as 1 in 5000 the contraction is very slight; whereas a solution of digitalin of 1 in 100,000 to 1 in 20,000 in normal saline rapidly reduces and arrests the flow through the blood-vessels.

The predominant action of acokantherin is that exerted upon striped muscle, and, because of this action, with possibly an action upon the intrinsic cardio-motor ganglia, the chief effect is produced upon the heart, while the influence exerted upon the cardio-respiratory centres in the medulla is relatively slight or secondary.

- IV. "The Development of *Asterina gibbosa*." By E. W. MACBRIDE, B.A., Demonstrator of Animal Morphology to the University of Cambridge. Communicated by A. SEDGWICK, F.R.S. Received March 18, 1895.

[For Abstract of this Paper see vol. 54, p. 431—"The Organogeny of *Asterina gibbosa*."]

*Presents, April 25, 1895.*

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May 2, 1895.

The LORD KELVIN, D.C.L., LL.D., President, followed by Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Vice-President and Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

|                                     |                                 |
|-------------------------------------|---------------------------------|
| Barry, J. Wolfe, M.Inst. C.E., C.B. | Holden, Henry C. L., Major,     |
| Bourne, Professor Alfred Gibbs,     | R.A.                            |
| D.Sc.                               | McClean, Frank, M.A., LL.D.     |
| Bryan, George Hartley, M.A.         | MacEwen, Professor William,     |
| Eliot, John, M.A.                   | M.D.                            |
| Green, Professor Joseph Reynolds,   | Martin, Sidney, M.D.            |
| D.Sc.                               | Minchin, Professor George M.,   |
| Griffiths, Ernest Howard, M.A.      | M.A.                            |
| Heycock, Charles Thomas, M.A.       | Power, William Henry.           |
| Hickson, Professor Sydney John,     | Purdie, Professor Thomas, B.Sc. |
| D.Sc.                               |                                 |

The following Papers were read:—

- I. "Helium, a Gaseous Constituent of certain Minerals. Part I." By WILLIAM RAMSAY, F.R.S., Professor of Chemistry in University College, London. Received April 27.

§ 1. *Introductory.*

IN seeking for a clue which would guide to the formation of compounds of argon, Mr. Miers, of the British Museum, kindly informed me that a gas, supposed to be nitrogen, was obtainable from certain minerals containing the metal uranium, and notably from clèveite, a Norwegian mineral discovered by Nordenskjöld. The gas evolved from a number of such minerals had been examined by Dr. W. F. Hillebrand, of the United States Geological Survey, and was pronounced by him to be nitrogen. The gas, which is evolved from some uraninites by the action of weak sulphuric acid, or by fusion with sodium carbonate, gave, in the hands of Dr. Hillebrand and Dr. Hallock,\* the usual fluted nitrogen spectrum, and on sparking with hydrogen in presence of acid, or with oxygen in presence of alkali, contraction ensued. From the first of these experiments they were able to prepare a weighable quantity of ammonium platinichloride.

In a private letter from Dr. Hillebrand, from which he allows me to quote, he states:—

"The circumstances and conditions under which my work in this line was done were unfavourable; the chemical investigations had consumed a vast amount of time, and I felt strong scruples about taking more from regular routine work. I was a novice at spectroscopic work of this kind, and was thereby led to attach too little importance to certain observations which in the light of your discoveries deserved the utmost consideration. Among other things, it puzzled me that the formation of ammonia should have proceeded so slowly, and that only a comparatively small amount of the gas should have been converted at the expiration of even several days' passage of the current. But, having had no previous experience, I was unable to judge whether this was especially abnormal or not; it might have been due to insufficient current, or to some other cause. A similar observation applies to the sparking with oxygen, which was only carried far enough to prove a contraction, and to obtain the tests for nitric acid.

"It doubtless has appeared incomprehensible to you, in view of the bright argon and other lines noticed by you in the gas from clèveite,

\* "On the Occurrence of Nitrogen in Uraninite, &c.," by W. F. Hillebrand, Bull. No. 78, U. S. Geological Survey, 1889-90.

that they should have escaped my observation. *They did not.* Both Dr. Hallock and I observed numerous bright lines on one or two occasions, some of which, apparently, could be accounted for by known elements—as mercury, or sulphur from sulphuric acid; but there were others which I could not identify with any mapped lines. The well-known variability in the spectra of some substances under varying conditions of current and degree of evacuation of the tube, led me to ascribe similar causes for these anomalous appearances, and to reject the suggestion made by one of us in a doubtfully serious spirit, that a new element might be in question.”

It appeared to me unlikely that any substance should give free nitrogen on treatment with an acid; and on examining a sample of gas evolved from clèveite, which I procured from Mr. Gregory, the conclusion was confirmed.

The mineral is imbedded in felspathic rock, and forms black nodules and veins in the light reddish felspar. A considerable quantity of the pure mineral could be picked out; but it was found more advantageous to powder the whole rock and to boil it with dilute sulphuric acid (1 to 8). Under this treatment the mineral rapidly disintegrates, and an abundant evolution of gas takes place, even in the cold.

The gas is most conveniently collected by the device shown in the accompanying woodcut. The mineral is placed in the flask A, which is exhausted of air by means of a Töpler pump; to expel the last traces of air, some water is admitted so as to moisten it. Acid which has been boiled is run in hot through the funnel B, and the connexion with the gas-holder, C, is opened. After half an hour's boiling with the mineral all gas had been evolved. The stop-cock communicating with the gas-holder was closed, the remaining gas taken out by means of the air-pump, and restored to the gas-holder through the arrangement depicted at D.

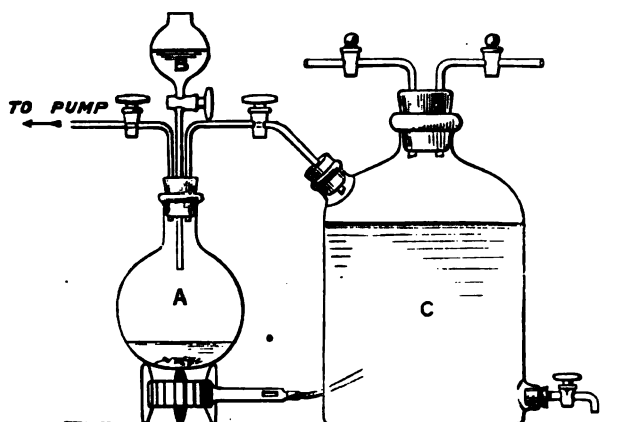
The gas-holder is, of course, filled with water, thoroughly boiled *in vacuo*, so as to avoid contaminating the gas with nitrogen, oxygen, or argon.

The method of extracting the gas on a small scale is practically identical, the difference consisting solely in the size of the vessels employed.\*

## § 2. *Preliminary Experiments.*

These experiments are described in the Preliminary Note communicated to the Society on March 26, 1895. I shall only add that it appears pretty certain, in the light of subsequent experiments, that

\* For preliminary experiments on the extraction of gas, my thanks are due to Mr. Donald Matthews. Mr. Morris Travers, my assistant, has kindly carried out all subsequent experiments on the extraction of gases.



this sample of gas was largely contaminated with atmospheric air. On sparking with oxygen in presence of soda, the nitrogen was, of course, removed; but it will be seen subsequently that the complete argon spectrum is not visible in the new gas.

### § 3. Sources of Helium.

I owe to the kindness of Dr. Hillebrand some grams of the uraninite which served for his experiments. It is a hard, black mineral, containing, according to his account (*loc. cit.*), the oxides of uranium, lead, thorium, yttrium, and allied metals in smaller amount. It is attacked by acid with great difficulty, and gas is very slowly evolved. A quantitative experiment was made by boiling 2.808 grams for  $5\frac{1}{2}$  hours, when 24 c.c. of gas were evolved. After further boiling for ten hours, an additional 13.5 c.c. were obtained, in all 37.5 c.c.

The first portion of this gas was mixed with oxygen and sparked for a short time. The volume of the mixture was 34.3 c.c., and after sparking it had diminished to 33.9 c.c. This appears to show that very little nitrogen is present if care be taken in collecting the gas. I must reserve to the second part of this paper a further account of this specimen.

The clèveite which I bought from Mr. Gregory is much more easily decomposed, for in the course of half an hour the mineral is completely disintegrated, and all gas is evolved. Mr. Travers found that 3.662 grams of the pure black mineral, free from felspar, yield 26 c.c. of gas.

This gas when mixed with oxygen and sparked did not contract to any great extent. The actual measurements are:—

|                                |           |
|--------------------------------|-----------|
| Taken .....                    | 24.0 c.c. |
| After addition of oxygen ..... | 34.0 „    |
| After sparking .....           | 32.5 „    |

It appears therefore that this gas contains some, but not much nitrogen, and practically no free hydrogen. It must be concluded that the hydrogen obtained in such large quantity from the crude felspathic mineral (see below) must have been derived from some free metal which it contained.

An attempt was also made to extract gas from Cornish pitchblende, a large sample of which I owe to the kindness of Mr. Kitto. It is decomposed by acid very slowly, and it also evolves gas when heated *in vacuo*, but in exceedingly small quantity. From 50 grams only a few cubic centimetres are evolved.

Similar results were obtained with a sample of pitchblende of unknown origin which I happened to possess. The amount of gas obtained was quite insignificant.

#### § 4. *Density of various Samples of Gas.*

It has been stated in the preliminary note already referred to, that the spectrum of the gas extracted from clèveite is characterised by a brilliant yellow line, which, according to Mr. Crookes' measurement, has the wave-length 587.49 millionths of a millimetre, and is identical with the line D<sub>3</sub>, to which Mr. Lockyer many years ago gave the name "helium," from its occurrence in the spectrum of the sun's chromosphere. As the line D<sub>3</sub> is observed high in the chromosphere, it has been inferred that the density of the gas should be very low. Special interest therefore attached to the determination of this important property.

The gas used for the first experiment was the crude gas from the felspathic rock containing clèveite.

About 300 c.c. confined over strong caustic potash solution, which has been well boiled to expel all air, were used. The globe, which was the same one that served for weighing argon, had a capacity of 162.843 c.c. The data are as follow :—

|                                     |              |
|-------------------------------------|--------------|
| Volume of gas .....                 | 162.843 c.c. |
| Pressure (corrected) .....          | 756.5 mm.    |
| Temperature .....                   | 17.0°        |
| Weight .....                        | 0.0212 gram. |
| Weight of 1 litre at 0° and 760 mm. | 0.1389 „     |
| Density (O = 16) .....              | 1.555        |

This gas, as will be afterwards explained, contained free hydrogen. In order to purify it from this constituent, it was passed from one gas-holder to another several times over red-hot copper oxide. The

total quantity of gas employed in this way was 920 c.c., and the volume of the residue, unaltered further by copper oxide, was 183 c.c. This large-scale experiment shows the gas to contain about 80 per cent. of hydrogen.

The density of the partially purified gas was then determined

|                                     |              |
|-------------------------------------|--------------|
| Volume of gas .....                 | 162·843 c.c. |
| Pressure (corrected) .....          | 757·6 mm.    |
| Temperature .....                   | 21·1°.       |
| Weight .....                        | 0·0652 gram. |
| Weight of 1 litre at 0° and 760 mm. | 0·4337 „     |
| Density (O = 16) .....              | 4·84         |

A vacuum-tube filled with this sample of gas showed strong flutings of nitrogen, and it was therefore further purified by repeated passage over red-hot magnesium, copper, copper oxide, soda-lime, and phosphorus pentoxide. This sample gave the result:—

|                                     |              |
|-------------------------------------|--------------|
| Volume of gas .....                 | 162·843 c.c. |
| Pressure .....                      | 704·7 mm.*   |
| Temperature .....                   | 17·62°       |
| Weight .....                        | 0·0510 gram. |
| Weight of 1 litre at 0° and 760 mm. | 0·3472 „     |
| Density (O = 16) .....              | 3·89         |

This gas, however, when examined spectroscopically, was seen still to contain nitrogen, but obviously the amount was diminished. I must reserve to a later date a more accurate determination. But judging by a preliminary experiment, in which a small quantity of this gas was sparked with oxygen until no further contraction occurred, and until it gave a spectrum absolutely free from all trace of nitrogen, no great reduction in the density is to be expected. The number 3·9 may, therefore, be taken as a maximum number for the density of helium.

#### § 5. *Wave-length of Sound in the Gas Purified from Hydrogen, and Ratio of Specific Heats.*

The tube used and the method employed for this experiment were the same as those used for argon by Lord Rayleigh and myself.† The half-wave-length of air in the tube at 15° was 34·45 mm.; that of the new gas (sample of density 4·84) as a mean of twelve readings was 69·33 at 13·6°. This gives a ratio of 1·863, which is obviously too high. But it must be remarked that the heaps of lycopodium,

\* As the stock of gas was too small to fill the globe and permit the requisite manipulation, a gauge was connected with the globe; and it was filled under reduced pressure.

† 'Phil. Trans.,' A, 1895.

although some were well marked, were not symmetrically arranged. Every attempt was made by altering the length of the india-rubber tubing which connected the wave-length tube with the supply of gas, to induce appropriate echoes. This plan works well with denser gases. But I found it of no avail. The half-wave-lengths were always between 67 and 71 mm., and there appears no reason which would justify choosing the lower figure, which would give the ratio of specific heats 1:1.74.

One conclusion, however, may be drawn: that the gas has a high ratio of specific heats, and that it is probably monatomic, if this inference is legitimate for argon.\*

#### § 6. *Is the Gas evolved from Clèveite a Hydride or an Element?*

It was soon discovered that the gas evolved from clèveite by treatment with dilute sulphuric acid contained a not inconsiderable amount of hydrogen. Now if the element helium is in combination in clèveite with the uranium, or the lead, or the yttrium, it is to be expected that it will be evolved as a hydride. It is true that a hydride might be unstable, in which case free hydrogen should be mixed with the helium.

An attempt was made to decide which alternative is correct by absorbing the free hydrogen by palladium. A measured quantity of the gas was treated with palladium sponge, hammered on an anvil, so as to avoid the introduction of air into the gas as much as possible, and it was passed up into the tube containing the gas. The numbers obtained are these:—

|                                        |            |
|----------------------------------------|------------|
| Original volume of crude gas. ....     | 12.20 c.c. |
| Residue after absorption of hydrogen.. | 2.40 „     |
| Hydrogen absorbed .....                | 9.80 „     |

The gas therefore consisted of 19.7 per cent. of a substance unattacked by palladium, after standing for a night in contact with it, and 80.3 per cent. of hydrogen. The residue was transferred to a sparking-tube, and sparked with oxygen over caustic potash. The volume then decreased to 2.20 c.c., and the residue is 18.0 per cent. of the total. The contraction is too small to allow of the hypothesis that a hydride of helium has been decomposed on sparking, although if the formula of the hydride were  $\text{HHe}$ , decomposition and removal of hydrogen might take place without change of volume.

#### § 7. *The Spectrum of the Gas from Clèveite.*

A careful comparison was made between the spectra of various samples of gas from clèveite and the spectrum of argon. As my spectroscope does not admit of accurate measurements being taken, I

\* 25th June, 1895. This conclusion has been amply confirmed.

had to content myself with throwing the two spectra into the same field of vision. By using two coils and adjusting current until the luminosity of the common lines in the two tubes becomes equal, very accurate conclusions can be drawn regarding the spectra.

A description has already been given of the first sample of gas examined (see Preliminary Note). In all probability a considerable amount of air had leaked in during its extraction, for it showed practically the whole of the argon spectrum in addition to lines to be ascribed to helium.

The gas prepared on a large scale, which consisted chiefly of hydrogen, was first examined. There is nothing in the spectrum which calls for special notice, unless it be that the helium yellow ( $D_3$ ) and violet lines were visible, even in presence of 80 per cent. of hydrogen. These and other helium lines were brought out more intensely after a large portion of the hydrogen had been removed by means of palladium. The argon red pair were, however, invisible in the helium-tube.

On sparking this gas, all hydrogen and nitrogen were removed. And after a vacuum-tube had been filled a careful comparison was made between the spectrum of this gas and that of argon.

Comparison of the Spectra of Helium and Argon.

| Description.                      | Helion. | Argon.     | Remarks.                                                                 |
|-----------------------------------|---------|------------|--------------------------------------------------------------------------|
| Extreme red, faint triplet ..     | Absent  | Present    | Stronger in helium.                                                      |
| Red; first line of first pair..   | Present | "          |                                                                          |
| " second " " ..                   | Absent  | "          |                                                                          |
| " first faint line of second pair | "       | "          |                                                                          |
| " second.....                     | Present | Very faint | Brilliant in helium; one of second pair of argon, but very faint.        |
| Orange-red, faint.....            | "       | Present    | Almost invisible in argon. " $D_3$ ," the characteristic line of helium. |
| Yellow, very brilliant.....       | "       | Absent     |                                                                          |
| Green, four lines.....            | Absent  | Present    | A very strong line.                                                      |
| Blue-green pair .....             | "       | "          |                                                                          |
| Blue-green, faint.....            | Present | Absent     |                                                                          |
| Peacock-green, brilliant....      | "       | "          |                                                                          |
| Blue-green, fairly strong...      | "       | "          |                                                                          |
| Blue-green, faint.....            | "       | Present    |                                                                          |
| Blue-green, very faint.....       | "       | Absent     |                                                                          |
| Blue, faint.....                  | "       | "          |                                                                          |
| Bright blue .....                 | "       | "          |                                                                          |
| Blue-violet, faint.....           | Absent  | Present    |                                                                          |
| Violet, brilliant.....            | Present | Absent     | Characteristic line.                                                     |
| Violet, faint.....                | "       | "          |                                                                          |
| Violet, faint pair.....           | "       | Present    |                                                                          |
| Violet, faint pair.....           | Absent  | "          |                                                                          |
| Violet, faint.....                | Present | "          |                                                                          |
| Violet, faint.....                | Absent  | "          |                                                                          |

This table may be taken as giving the most striking lines of helium. Those which at once catch the eye are first the yellow line  $D_3$ , which enormously surpasses the rest in brilliancy. Next in intensity, but still brilliant, is the violet line; next the peacock-green, and finally the red. All other lines are comparatively faint.

That the argon spectrum is reproduced with variations in the helium-tube might lead to the conclusion that argon is present in the helium. But that argon was derived from atmospheric sources or from the water is excluded by the method of extraction. Even supposing that a few cubic centimetres of air had entered by leakage, the amount of argon contained therein, compared with the large volume of the helium, would make it practically impossible that the spectrum of argon should be visible. But there is no reason to suspect such a leakage. On the other hand, the helium spectrum reproduces one of the characteristic red lines of argon more intensely than it is seen in the argon-tube; and the other is absent. The faint orange-red line,\* a faint blue-green, and three violet lines are also present in both. Can it be that both these gases are mixtures? That there is no helium, or at least no gas giving the line  $D_3$  in atmospheric air is certain, for its brilliancy would lead at once to its detection in the spectrum of atmospheric argon if it were present.

#### § 8. *General Remarks.*

So far as these experiments go, they show that a gas can be extracted from clèveite, and probably from some allied minerals, lighter than any known gas except hydrogen. Whether or not this gas is a mixture or a single substance remains undecided. The fact of its spectrum possessing some of the lines of argon and not others may point to the conclusion that argon is a mixture, but until some methods of fractionation have been tried it is premature to speculate. Only one remark may be permitted of a speculative nature: the general similarity of helium (applying this name to the gas from clèveite) to argon, in not being affected either by red-hot magnesium or by sparking with oxygen in presence of potash, makes the inference probable that they belong to the same natural group. If the atomic weight of argon be 20 (a supposition which may be supported by some lines of argument), then, on subtracting 16, which is the average difference between the atomic weights of members of the first line, beginning with lithium and continued to fluorine, and the second line, beginning with sodium and ending with chlorine, the number 4 is obtained; and this closely approximates to the found density of helium, if that number is not too high. On the other hand,

\* Subsequent experiments have shown that this line is strong in helium near the negative electrode.

if the atomic weight of argon be 40, on subtracting 32, or twice the average difference, the number 8 is obtained, which closely approximates to  $3.9 \times 2$ . Which of these views is correct time must decide.

II. "Alternate Current Dynamo-Electric Machines." By J. HOPKINSON, F.R.S., and E. WILSON.\* Received April 4, 1895.

(Abstract.)

The paper deals experimentally with the currents induced in the coils and in the cores of the magnets of alternate current machines by the varying currents in and the varying positions of the armature. It is shown that such currents exist, and that they have the effect of diminishing to a certain extent the electromotive force of the machine when working on resistances as a generator without a corresponding effect upon the phase of the armature current. It is also shown that preventing variations in the coils of the electromagnet does not, in the machine experimented upon, greatly affect the result, and that the effect of introducing copper plates between the magnets and the armature has not a very great effect upon the electromotive force of the armature, the conclusion being that the conductivity of the iron cores is sufficient to produce the main part of the effect. A method of determining the efficiency of alternate current machines is illustrated, and the results of the experiments for this determination are utilised to show that in certain cases of relation of phase of current to phase of electromotive force, the effect of the local currents in the iron cores is to increase, instead of to diminish, the electromotive force of the machine.

III. "Note on the Relations of Sensory Impressions and Sensory Centres to Voluntary Movements." By H. CHARLTON BASTIAN, M.D., F.R.S., Professor of Clinical Medicine in University College, London. Received April 5, 1895.

In a recent communication to the Royal Society by Drs. Mott and Sherrington, entitled "Experiments upon the Influence of Sensory Nerves upon Movement and Nutrition of the Limbs," results of a most important and hitherto unsuspected character were brought forward. In this communication they have shown that "section of the whole series of sensory roots belonging to a limb," either upper

\* The large majority of the experiments herein described were made in the summer of 1893, and a considerable part of the paper was then written. We have to thank Mr. F. Lydall, one of the student demonstrators at King's College at that time, for much assistance.

or lower, immediately produces a lasting motor paralysis in the limb thus apæsthete.

The interpretation of these results seems at first sight very difficult. The authors of the paper, after referring to views which I have put forward in reference to the fundamental importance of sensory impressions in the production of voluntary movements, give expression to the following explanation:—"We think these experiments go even further than his arguments in pointing to the influence of sensation upon voluntary movement, inasmuch as they indicate that not only the cortex, but the whole sensory path from periphery to *cortex cerebri*, is in action during voluntary movement."

This interpretation of their interesting experimental results is one which I cannot accept because it is opposed to other thoroughly ascertained facts. Clinical investigation has established the fact that in cases of complete hemianæsthesia due to lesions or functional defects in the posterior part of the internal capsule, there is not only no paralysis but little or no impairment in the ability to perform, under visual guidance, even the most delicate movements with the apæsthete limbs. This shows therefore that it is not necessary, as they suggest, for "the whole sensory path from periphery to *cortex cerebri*" to be in action during the performance of voluntary movements. It was the knowledge of these facts, made known by clinical investigation, that caused me to be much perplexed when Dr. Sherrington kindly showed me some of the animals on which he had been experimenting. The reality of the paralysis was obvious, but how were we to account for the fact that the interruption of the sensory channels from a limb in one part of their course produced this almost complete paralysis, whilst the interruption of these channels higher up (in the brain itself) gave rise to no such results? I was for a time quite unable to explain the apparent discrepancy, though further consideration seemed to remove the difficulty.

A brief exposition will, I hope, suffice to throw light upon this question, as well as upon the extent to which afferent impressions and the activity of their related centres are really needed for the production of voluntary movements.

I have for some years contended, in opposition to very generally accepted views, that there is no evidence to prove the existence of motor centres in the cerebral cortex; whilst, on the other hand, there is much evidence to show that the cortical regions supposed to be motor, are, in reality, sensory centres of kinæsthetic type.\* The postulation of the existence of motor centres in the cerebral cortex had its origin in, or, at least, derived its principal support from, the

\* "The Muscular Sense: its Nature and Cortical Localisation," 'Brain,' April, 1887.

doctrine (now generally admitted to be erroneous) that "feelings of movement" were, in the main, "concomitants of the out-going current."

Further, I have endeavoured to show that sensory impressions and the activity of sensory centres are the real guides for volitional action, that they, in fact, do just such work as has been attributed to supposed cortical motor centres, and that it is a fundamental error to imagine that cortical motor centres exist for the performance of voluntary movements, altogether apart from the other motor centres that are concerned with the production of reflex or secondary automatic acts.\*

I have contended, in short, that true motor centres exist only in the pons bulb and spinal cord, which may be called into activity in different ways, according as the stimulus comes, in the one case (*a*) from the cortex, for volitional movements; or as it comes (*b*) through afferent nerves and lower sensory centres, as in reflex acts.

In regard to movements of the latter category (*b*), there is no room for difference of opinion, and the results of the experiments of Drs. Mott and Sherrington are thoroughly in harmony with what is generally admitted. Section of all the sensory roots proceeding from a limb must abolish, as it has been found to do, all reflex movements in this limb.

We may turn therefore at once to the consideration of (*a*) movements initiated from the cerebral cortex (so-called voluntary movements), and strive to ascertain in what various modes such movements may be rendered impossible, or, in other words, how paralysis of such movements may be occasioned.

It may be noted here that the writer was the first, in 1869, in opposition to then prevalent physiological notions, to postulate the existence of various sensory centres in the cerebral cortex,† and that some years before any experimental evidence was brought forward on the subject. About the same time he showed how this hypothesis sufficed to throw light upon the nature of various forms of speech defects,‡ and in this he was followed by Broadbent,§ still before any attempt had been made to localise such centres.||

\* "On the Neural Processes underlying Attention and Volition," 'Brain,' April, 1892.

† "On the Localisation of Function in the Cerebral Hemispheres," 'Journ. of Ment. Science,' January, 1869; and "On the Muscular Sense and the Physiology of Thinking," 'Brit. Med. Journ.,' May, 1869.

‡ "Physiology of Thinking," 'Fortnightly Review,' January, 1869; and "Defects of Speech in Brain Disease," 'Brit. and For. Med. Chir. Review,' January and April, 1869.

§ "On the Cerebral Mechanism of Speech and Thought," 'Med. Chir. Trans.,' 1872, p. 180.

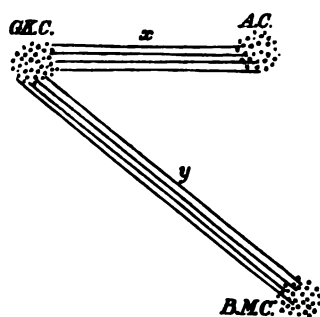
|| See Ferrier in 'Phil's Trans.,' Part II, 1875.

From the point of view of sensory guidance, cortically initiated movements are divisible into two categories:—(I) *Speech movements*, which are now known to be produced under the guidance of co-active auditory and kinæsthetic centres; and (II) *Limb and other bodily movements*, which are equally well known to be produced under the guidance of co-active visual and kinæsthetic centres. In each case the kinæsthetic centres do not seem to act independently, but only in response to the primarily initiated activity in the auditory or in the visual centres respectively—these latter in all cases seeming to take the lead.

I. *Speech Movements*.—Words appear to be revived in thought, in the main, in the auditory centres, and, if they are to be spoken, stimuli from these centres must pass along associational fibres to related portions of the kinæsthetic centre, that is, to what I have termed the “glosso-kinæsthetic” centre, in and near the posterior part of the third frontal convolution. If we assume, as clinico-pathological evidence seems to warrant, that the portion of the auditory centre concerned with the registration of the sounds of words is situated in the posterior part of the upper temporal convolution, the associational fibres in question would pass in their course from the auditory centre beneath the Island of Reil in order to reach the glosso-kinæsthetic centre. From this region the combined stimuli would pass off from the cortex (and through the internal capsule), so as to evoke in appropriate ways the activity of the proper motor centres for speech situated in the bulb (fig. 1).

It has now been ascertained that speech movements may be paralysed by lesions in any part of this tract from the auditory centre to the bulb. Lesions in either of the two sensory centres will pro-

FIG. 1.—Diagram illustrating relative Positions of three Cerebral Centres concerned with Speech.



AC, auditory centre; GKC, glosso-kinæsthetic centre; BMC, bulbar motor centre; *x*, audito-kinæsthetic commissural fibres; *y*, internuncial fibres.

duce such paralysis just as certainly as lesions in the bulbar motor centres themselves. If the lesion be situated in the auditory word centre (AC) it will produce "word-blindness" as well as loss of speech; though if it be situated in the glosso-kinæsthetic centre (GKC) it will produce loss of speech alone. In opposition, however, to the doctrine as to the strict localisation of aphasia, as a symptom resulting only from lesions in the third frontal convolution (Broca's convolution), I have long maintained that precisely similar defects would result from destruction of the commissural fibres ( $x$ ) in any part of their course—thus affording an explanation of the many cases recorded by Meynert and others in which aphasia had been caused by a lesion in the "Island of Reil."

Again, it has long been known that damage to the internuncial fibres ( $y$ ) in any part of their course would also cause loss of speech (*i.e.*, paralysis of speech movements), similar in kind to that caused by lesions in the bulbar motor centres (BMC).

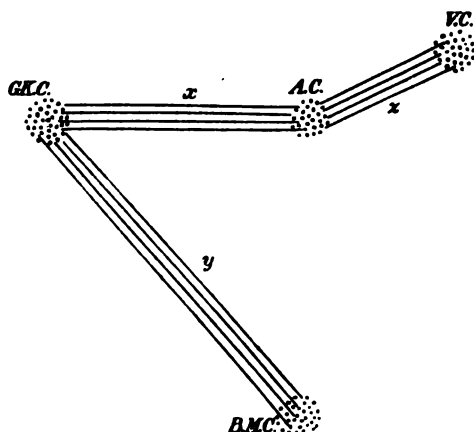
In what has been said above we have illustrations of the importance of afferent impressions and the revived activity of afferent centres for the production of the voluntary movements occurring in articulate speech. And if, instead of looking to the processes which occur when our own thoughts reveal themselves in spontaneous speech, we turn to what happens when we read the recorded thoughts of another, it will be found that a further relation with sensory centres has to be established before such reading aloud can occur.

The impressions from the printed page impinge upon the visual centre (fig. 2, VC), and are thence transmitted across commissural fibres to related portion of the auditory centre, whence the stimuli pass, as in spontaneous speech, to the glosso-kinæsthetic centre and thence on to the bulb. The consequence is, that where lesions occur in the course of these visuo-auditory fibres ( $z$ ), the individual is unable to read aloud, and he is unable to name objects or even single letters, although he will immediately repeat words or letters as soon as he hears them. He cannot carry out the voluntary movements concerned with speech at the instigation of the visual sense, though he can do so at the instigation of auditory impressions. We get thus at the roots of Will and find them definitely localised in sensory centres.\*

II. *Limb Movements.*—The visual sense in the case of limb movements performs the same office as the auditory sense in the case of speech. New movements of limbs are learned largely by aid of the visual sense, supplemented by associated kinæsthetic impressions. So that subsequently, when we desire to repeat familiar movements,

\* Cases illustrating the effects of lesions to the commissures between the auditory and the visual word centres may be found recorded in 'The Brain as an Organ of Mind,' 4th edition, pp. 640—647.

FIG. 2.—Diagram illustrating relative Positions of four Cerebral Centres concerned with Reading aloud.



VC, visual centre; *x*, visuo-auditory commissural fibres. Other references as in Fig. 1.

such desire is accompanied by a "conception" of the movement to be performed. This "conception," in physiological terms, means a revival in sub-conscious memory of the visual and kinæsthetic impressions pertaining to the movement in question.

Limb movements, like others, may be paralysed either by organic lesions or by functional defects.

#### A. Organic Lesions.

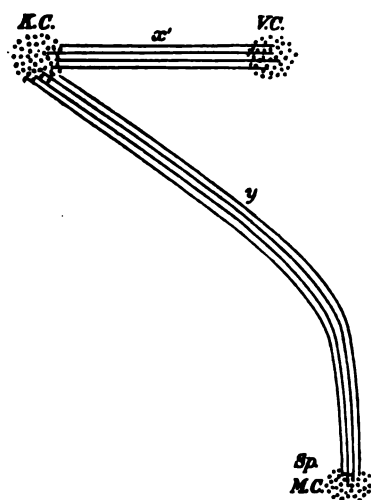
If the kinæsthetic centres in relation with the limb are destroyed, paralysis of the limb results, together with loss of muscular sense and kinæsthetic impressions generally. This has now been determined by clinico-pathological evidence, as well as by the results following excision of portions of the cortex by reason of disease occurring therein.

As yet we have no evidence from man of paralysis of limb movements following extensive disease of the visual centre, or of the commissures existing between it and the kinæsthetic centres for the limbs, comparable with the paralysis of speech movements following disease of the auditory centre or the audito-kinæsthetic commissural fibres, except in the case of writing movements. It is, however, certainly true that with destruction of the left visual word-centre the individual is no longer able to write words or even a single letter.

But it would seem that some experimental evidence on lower

animals does exist, tending to show that a severance of the visuo-kinæsthetic fibres (fig. 3, *x*) is followed by the same kind of paralysis

FIG. 3.—Diagram illustrating relative Positions of two Cerebral Sensory and one Spinal Motor Centre concerned with Writing Movements.



VC, visual centre; KC, kinæsthetic centre; SpMC, spinal motor centre; *x'*, visuo-kinæsthetic commissural fibres; *y*, internuncial fibres.

of limbs as that which results from destruction of the kinæsthetic centres themselves. Thus, Marique\* has found, and his results have been confirmed by Exner and Paneth,† that isolation of the kinæsthetic centres, by section of the fibres that connect them with the other sensory centres of the cortex, produces a paralysis of precisely the same character as that which occurs when these so-called motor centres are extirpated. Marique, moreover, found that the same kind of muscular contractions were produced on electrical irritation of the respective kinæsthetic centres after, as before, isolation, showing that these centres still retained their excitability and their connection with the pyramidal tracts.

#### B. *Functional Defects.*

Functional defects leading to paralysis of limbs may be either (a) cerebral or (b) spinal in seat.

\* 'Centres Psycho-moteurs du Cerveau,' 1885.

† 'Archiv f. d. Ges. Phys.,' bd. 41, 1889.

(a.) *Cerebral Functional Defects*.—To this cerebral category belong cases of the so-called hysterical type, in which there appear to be temporary defects in the nutritional condition of the kinæsthetic centres leading to various temporary and curable forms of paralysis (either monoplegias, hemiplegias, or paraplegias), such as I have elsewhere described,\* always associated with a corresponding loss of muscular sense and, more or less, of defect in common sensibility. These forms of functional paralysis are often combined with well-marked complete hemianæsthesia, either single or double, presumably due to a co-existing nutritional defect in the sensory region of the internal capsule.

Strangely enough, in one class of these cases the patients are found to be capable of performing movements so long as their eyes are open, though they are quite unable to perform even the simplest movements with the affected limbs as soon as the eyes are closed;† results which are, I think, to be explained by the existence of a minor degree of that nutritional degradation of the kinæsthetic centres which, in the other class, produces paralysis whether the eyes are open or closed. The minor degradation would seem to permit of the kinæsthetic centres being roused by a slightly stronger stimulus coming to them from the visual centres of a person whose eyes are open, though they are no longer capable of being roused by the hypothetically weaker stimulus coming from the visual centres of a person whose eyes are closed.

(b.) *Spinal Functional Defects*.—It is into this category that, in my opinion, the forms of paralysis fall that have been produced by Drs. Mott and Sherrington as a result of section of all the sensory roots of a limb.

We have been long familiar with forms of paralysis due to structural lesions affecting the great ganglion cells in the anterior cornua of the cord; those, for instance, due to polio-myelitis or to focal lesions of different kinds involving the anterior cornua.

Three years ago, moreover, I gave reasons for believing that there are cases of functional paralysis of spinal type, due to defects in these same regions of the spinal cord (examples of which were recorded), that ought to be distinctly separated from those of cerebral origin commonly known as “hysterical.”‡ We have now, as I am inclined to maintain, in the experiments of Drs. Mott and Sherrington, an experimental proof of the existence of one of these forms of functional paralysis of spinal origin.

Instead of a lowered functional activity in cerebral kinæsthetic centres (interfering with volition), we have here a lowered functional

\* ‘Various Forms of Hysterical or Functional Paralysis,’ London, 1893.

† *Loc. cit.*, pp. 22–28.

‡ *Loc. cit.*, pp. 96–116.

activity in the motor centres themselves situated in the spinal cord by reason of which their molecular activity is so altered that they are no longer capable of responding to ordinary volitional stimuli coming from the cerebral cortex. This, at least, is the explanation that I would suggest, instead of supposing, as the authors do, that volitional power itself "has been absolutely abolished by the local loss of all forms of sensibility" in the paralysed limbs.

As I have already stated, a local loss of all forms of sensibility caused by a lesion in the brain in the region of the internal capsule causes no such paralysis. There is therefore no ground for supposing that the animal's power of willing is interfered with by section of all the posterior roots coming from the limbs; while, on the other hand, all the details furnished by Drs. Mott and Sherrington are quite in harmony with the interpretation that the animal's ordinary will power is unable to excite the spinal motor centres to action when their molecular condition has been altered by cutting off all the different stimuli proceeding to the corresponding region of the cord. The immediate result of this may be presumed to be a lowering of the habitual sub-activity of the motor centres upon which the condition of *tonus* in the muscles depends.

The fact that the results are produced only when *all* the sensory roots are cut, and that "the defect in motility increases from the attached base to the free apex of the limb," so that "the independent and more delicately adjusted movements which employ preponderantly the smaller and more individualised muscular masses of the hand and foot" are those which are most severely affected or abolished, although they may appear confirmatory of the interpretation of Drs. Mott and Sherrington, are, in fact, no less in accordance with that here given. This will be seen to be so if we bear in mind the overlapping of the fields of distribution of the sensory roots in the spinal cord (as shown by Sherrington), and the fact that the most delicate stimuli going to the smaller muscles might be expected to be those which would prove most impotent to rouse the sluggish spinal centres into activity. Again, when the authors say: "We find, however, that forcible and rapid movements, even of the fine joints at the end of the limb, can be induced in the animals by causing them to 'struggle;' for instance, while recovering from ether inhalation or while trying to free themselves on being held awkwardly, the whole limb at all its joints may exhibit movements," we have facts quite compatible with my interpretation. Muscles may not respond to ordinary volitional stimuli, and yet may respond when the stimulus is strengthened under the influence of emotion. In a similar manner may we explain the fact that when the kinæsthetic centres corresponding with an apæsthetic limb are stimulated by electricity, movements of the previously paralysed limb are produced just as easily as they are in a normal

limb.\* Such results may, as the authors say, be considered to point to the "profound difference between the production of the fine movements of the limb in volition, on the one hand, and by experimental stimulation of the cortex, on the other." This explanation is as valid for the one as for the other interpretation.

The stimulation by electricity may, and probably does, involve a very different kind of stimulus from that which normally emanates from the cortex during a voluntary act, and how subtle are the differences that exist between the conditions leading to paralysis or not in different instances may be judged from the functional cases of paralysis of cerebral origin, previously referred to, in which there may be in the same person, in immediately successive periods, complete paralysis of the limb so long as the eyes are closed, and no such paralysis when the eyes are opened.

What has been said above shows, moreover, how much removed we are from the position assumed to be true about twenty years ago,† when centres "immediately concerned in effecting volitional movements" were considered to be "as such truly motor."

#### IV. "The Fasciola Cinerea ; its Relation to the Fascia Dentata and to the Nerves of Lancisi." By ALEX HILL, M.D., Master of Downing College. Communicated by Prof. A. MACALISTER, F.R.S. Received April 2, 1895.

In my paper on the hippocampus, published in the *Philosophical Transactions of the Royal Society* for 1893 (vol. 184, B, pp. 389—429), I stated as a subsidiary conclusion, resulting from my investigation of the brains of marine mammals, that "there is no reason for associating the fascia dentata with the striæ longitudinales (nervus Lancisii), gyrus supracallosalis, and gyrus geniculi, or for supposing that all these four structures belong to a single organ, which forms a part of the cortical centre for the sense of smell."

This conclusion was based upon the following observations:—A. In

\* This fact shows that the cutting off of the afferent impressions by section of the sensory roots does not entail a lowered excitability of the kinæsthetic centres in the cortex, but rather the reverse, if we look to the really lowered activity in the spinal centres which the absence of tonus implies. In the case of complete cerebral hemianæsthesia, however, there is, over and above the absence of any such lowered activity of the spinal centres, another important difference between the conditions existing and those which obtain in the experiments of Drs. Mott and Sherrington, viz., no cutting off of cerebellar influence, so that the activity of the cortex and of the sub-cortical centres is less interfered with. These differences tend, I think, to explain the presence of paralysis with section of the posterior roots, and its absence with cerebral hemianæsthesia. (*May 4, 1895.*)

† Ferrier, "Functions of the Brain," 1st Ed., 1876, p. 200.

certain aquatic mammals completely destitute of olfactory apparatus, *e.g.*, *Hyperoodon* (bottle-nosed whale), the stria longitudinalis lateralis is present in its usual form, and enlarges anteriorly into supra-callosal and geniculate convolutions, which are unusually distinct, although minute. B. In megosmatic animals the fascia dentata is continued far beneath the corpus callosum, and only ceases to accompany the fimbria where this body turns downwards in the anterior pillar of the fornix. C. Coronal sections carried through the back of the corpus callosum of an ox-brain revealed no trace of a recurrent fascia dentata. On these grounds I assumed that the fascia dentata is a sub-callosal structure. In this conclusion I was, however, completely mistaken, as investigations in progress at the time when my paper on the hippocampus went to press have clearly proved.

*The fascia dentata is continued on to the dorsal surface of the corpus callosum.*

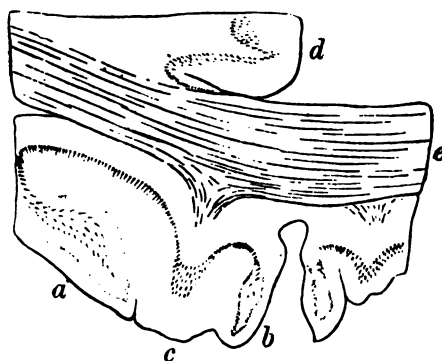
A series of sections carried through the corpus callosum with the underlying fimbria and fascia dentata, in the brain of a cat, show that, just as the fornix is turning downwards in its anterior pillar, the fascia dentata is folded on itself, and retraces its course back towards the splenium corporis callosi, almost along the middle line of the brain. In this folding the trough of fascia dentata, into which the pyramidal cells of the margin of the cortex are received, remains open dorsally; at the fold the margins of the trough are slanted somewhat forwards.

A coronal section taken a short distance behind the fold (fig. A) shows the fascia dentata cut twice. On the outer side it is large and well-developed; on the mesial side the recurrent fascia is small and narrow. Between the two parts of this folded ribbon is pushed a rounded tubercle of cortex (callosal convolution of Zuckerkandl), which is continuous with the gyrus fornicatus.

Slightly farther back the trough of fascia dentata is reduced to a curved plate (fig. B). This extends but a little way, for, at about half the distance between the folding of the fascia dentata and the splenium corporis callosi, the stratum granulosum, which has invested the margin of the cortex from the temporal extremity of the hiatus ventriculi to this point, comes to an end. Near the splenium the layer of pyramidal cells which represents the general cortex is reduced to a fold with a bevelled edge, and no indications of its retaining in its tissue any remnants of the characteristic granule layer of the fascia dentata are to be seen. My sections through the corpus callosum of the ox were not carried sufficiently far forward.

In a certain number of human brains the continuity of fascia dentata and stria longitudinalis through the medium of an intervening band, the fasciola cinerea, may be seen very distinctly.

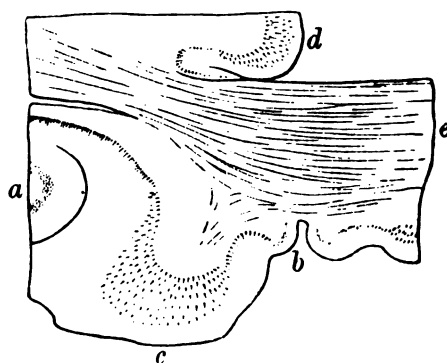
FIG. A.



Coronal Section through the Corpus Callosum and underlying Fascia Dentata of the Cat.

- a. Fascia dentata.
- b. Recurrent fascia dentata or fasciola cinerea.
- c. Callosal convolution.
- d. Gyrus fornicatus, with stria longitudinalis lateralis between it and the corpus callosum.
- e. Corpus callosum.

FIG. B.



Coronal Section through the Corpus Callosum and underlying Fascia Dentata nearer the Splenium Corporis Callosi than in figure A.

- a. Fascia dentata.
- b. Recurrent fascia dentata or fasciola cinerea.
- c. Callosal convolution.
- d. Gyrus fornicatus, with stria longitudinalis lateralis between it and the corpus callosum.
- e. Corpus callosum.

Fig. C is a photograph of a brain in which the fasciola cinerea is well-developed. The dentations of the fascia dentata as it lies in the dentary fossa are unusually well marked. As the fimbria leaves it

FIG. C.

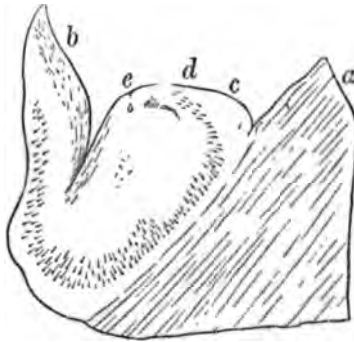


Photograph of a Portion of the Brain of a Man 45 years old. The Brain had been hardened in bichromate of ammonia. Magn.  $\frac{1}{4}$ .

- A. Gyrus fornicatus joining—
- B. Gyrus lingualis.
- C. Corpus callosum.
- D. Posterior pillar of the fornix. The fascia dentata is seen passing out of the groove between the fimbria and the gyrus hippocampi; it divides after a short distance into—
- E. Callosal convolution and—
- F. Fasciola cinerea, which ascends around the back of the splenium corporis callosi.

to pass on the ventral side of the corpus callosum, the fascia dentata (fasciola cinerea) ascends on the posterior surface of the splenium, at first as a band of some breadth (fig. D), which divides, after a course of about 4 mm., into two parts (fig. E), of which the outer is a small oval convolution (callosal convolution), and the inner the continuation of the fascia dentata. The cortex from the gyrus fornicatus is continued into the callosal convolution, in which it is closely folded like a capital Q before it enters the fasciola cinerea. In the fasciola cinerea it lies at first flat on the corpus callosum, and then folds back on itself, to receive its cap of stratum granulosum, which borders the groove that separates it from the callosal convolution. Traces of a cap of fascia dentata can be seen for about 2 mm. beyond the separation of the gyrus callosalis and the fasciola. For

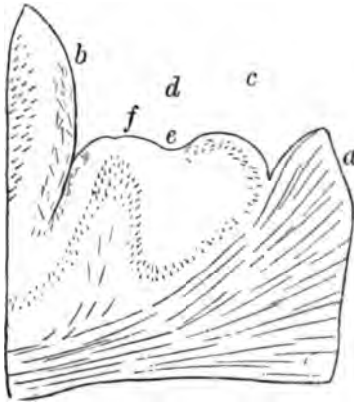
FIG. D.



Transverse Section through the Fasciola Cinerea of Brain C, just before the appearance of the little Callosal Convolution on the outer side of the fasciola.

- a. Corpus callosum.
- b. Gyrus fornicatus.
- c. Subiculum cornu Ammonis.
- d. Stratum granulosum.
- e. Groove between stratum granulosum and callosal convolution. A blood vessel is seen in this groove.

FIG. E.



Transverse Section through the Fasciola Cinerea and Gyrus Callosalis of Brain C.

The letters indicate the same structures as in figure D, with the addition of f, Gyrus callosalis.

some distance beyond the point at which the last "granules" disappear this folding outwards of the edge of the cortex is still visible.

In my paper on the hippocampus (p. 392) I have given an account of the views held by various anatomists as to the connections of the

*stria longitudinalis medialis* (nervus Lancisii) and *stria lateralis* (*stria oblecta*, *taenia tecta*). My preparations seem to prove that the mesial stria is the continuation of the fasciola cinerea, while the lateral stria belongs to the circumcallosal convolution, which is destroyed in most parts, although it may be developed in front into gyrus geniculatus, above into gyrus supracallosalis, and behind into gyrus callosalis (or sub-splenialis). It is, however, very difficult to follow the atrophied fascia dentata and circumcallosal convolutions through the indusium griseum which clothes the back of the splenium, and I prefer to wait for a brain in which these foetal structures may chance to be unusually well-developed, before expressing a decided opinion as to their homology.

- V. "An Attempt to Cultivate Parasitic Protozoa from Malignant Tumours, Vaccinia, Molluscum Contagiosum, and certain Normal Tissues, together with Infection Experiments carried out with the Culture Media, and a Note on the Treatment of Cancer." By SAMUEL G. SHATTOCK and CHARLES A. BALLANCE. Communicated by Sir JAMES PAGET, Bart., F.R.S. Received March 25, 1895.

[Publication deferred.]

*Presents, May 2, 1895.*

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Annaes de Sciencias Naturaes. Anno II. No. 2. 8vo. *Porto* 1895.

The Editor.

Archives Néerlandaises des Sciences Exactes et Naturelles. Tome XXIX. Livr. 1. 8vo. *Harlem* 1895.

Société Hollandaise des Sciences —

Jahresbericht über die Fortschritte in der Lehre von der Gährungs-Organismen. Jahrg. IV. 8vo. *Braunschweig* 1894.

The Publisher.

Medico-Legal Journal. Vol. XII. No. 3. 8vo. *New York* 1894.

The Editor.

Mittheilungen aus der Zoologischen Station zu Neapel. Bd. XI Heft 4. 8vo. *Berlin* 1895.

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Zeitschrift für Biologie. Bd. XXXII. Heft 1. 8vo. *München* 1895.

The Editors.

Bedell (F.) and Ryan (H. J.) Action of a Single-phase Synchronous Motor. 8vo. *Philadelphia* 1895.

The Authors.

Braithwaite (R.) The British Moss-Flora. Part XVI. 4to. *London* 1895.

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Iwanzoff (N.). Der mikroskopische Bau des elektrischen Organes von Torpedo. 8vo. *Moskau* 1895; [and two Excerpts. 8vo.]

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Jörgensen (A.) Der Ursprung der Weinhefen. 8vo. *Jena* 1895.

The Author.

- Kölliker (A. v.), For. Mem. R.S. Kritik der Hypothesen von Rabl-Rückhard und Duval über amoeboide Bewegungen der Neurodendren. 8vo. *Würzburg* 1895. The Author.
- Martini (T.) Egisto Tortori e l'Arte di modellare in Cera. 8vo. *Firenze* 1895. R. Istituto Tecnico, Florence.
- Outerbridge (A. E., jr.). A Lecture on "Matter," including "Radiant Matter." 8vo. [*Philadelphia* 1894.] The Author.
- Rajna (M.) Sull' Escursione Diurna della Declinazione Magnetica a Milano in relazione col Periodo delle Macchie Solari. 8vo. *Milano* 1895. The Author.
- Tortori (E.) Genesi, Organizzazione e Metamorfosi degli Infusori: Opera Postuma. 8vo. *Firenze* 1895. R. Istituto Tecnico, Florence.
- Wilde (H.), F.R.S. On the Evidence afforded by Bode's Law of a permanent Contraction of the *Radii Vectores* of the Planetary Orbits. 8vo. *Manchester* 1895; on the Multiple Proportions of the Atomic Weights of Elementary Substances in relation to the Unit of Hydrogen. 8vo. *Manchester* 1895. The Author.

May 9, 1895.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Bakerian Lecture was delivered as follows:—

BAKERIAN LECTURE.—“On the Laws of Connexion between the Conditions of Chemical Change and its Amount.” By A. VERNON HARCOURT, M.A., F.R.S., Student of Christ Church, and Lee’s Reader in Chemistry, and WILLIAM ESSON, M.A., F.R.S., Fellow of Merton College, and Deputy Savilian Professor of Geometry in the University of Oxford. “III. Further Researches on the Reaction of Hydrogen Dioxide and Hydrogen Iodide.” Received March 15, 1895.

(Abstract.)

In this paper are considered the effect upon the reaction of (1) substances not directly participating in it, (2) temperature.

A short description is given of the manner in which the observations were made.

The equation which expresses the result of a single set of observations was shown in a previous paper to be

$$y_1 = y_0 e^{-\alpha(t_1 - t_0)},$$

where  $y_0(H^2O^2 + 2HI = I^2 + 2H^2O),$

and  $y_1(H^2O^2 + 2HI = I^2 + 2H^2O),$

are the amounts of potential change at the times  $t_0, t_1$  respectively.

Each set of observations gives a mean value of  $\alpha$ , which is taken to be the value of the rate of change under the conditions of the set.

The effect of the variation of the conditions upon the value of  $\alpha$  is considered to be a measure of their effect upon the course of the reaction.

#### *Variation of Hydrogen Sulphate.*

It is shown from the results of three sets of experiments at temperatures of  $16^\circ, 20^\circ$ , and  $30^\circ$ , that the increment in the value of the

rate of change per unit of sulphate per unit of iodide is constant when an unit volume contains quantities of sulphate ranging from 45 to 515  $H^2SO^4$ .\* This increment is again constant for the range 515 to 762  $H^2SO^4$ , but has a higher value. For quantities of sulphate greater than 762  $H^2SO^4$  this increment, which again becomes constant, is further increased in value. The explanation given of this discontinuity in the successive values of the increment, is that the gradual addition of the sulphate to the water gives rise to the gradual formation of one hydrate at the expense of another, and the increment per unit of sulphate is the resultant of the increment per unit of each of the hydrates.

For the range 515 to 762  $H^2SO^4$  the hydrate which is decreasing, is  $H^2SO^4$ , 106  $H^2O$ , and the hydrate which is increasing, is  $H^2SO^4$ , 71  $H^2O$ .

At a temperature of  $30^\circ$  the values of  $\alpha$  in a solution containing in 1 c.c. 1  $HI$ ,  $sH^2SO^4$  are

$$(1) \text{ from } s = 45 \text{ to } s = 515,$$

$$\alpha = (1230 + 18.1s)10^{-6};$$

$$(2) \text{ from } s = 515 \text{ to } s = 762,$$

$$\alpha = \{1230 + 18.1 \times 515 + 22.4(s - 515)\}10^{-6};$$

$$(3) \text{ from } s = 762 \text{ to } s = 1140,$$

$$\alpha = \{1230 + 18.1 \times 515 + 22.4(762 - 515) + 26.5(s - 762)\}10^{-6}.$$

Theoretically the rate  $1230 \times 10^{-6}$  is the rate with water and hydrogen iodide only, present in amounts per cubic centimetre 55400  $H^2O$ , 1  $HI$ . The increments per unit of hydrogen sulphate are in the three cases 18.1, 22.4, and 26.5 millionths.

When the medium consists of water, hydrogen iodide, and hydrogen sulphate, and the ratio of the numbers of  $H^2SO^4$  and  $HI$  falls below 20, the rate with unit of  $HI$  has a value different from  $1230 \times 10^{-6}$ ; but the value of the increment per unit of sulphate is the same for the range of numbers of  $H^2SO^4$  from 45 to 515.

#### *Variation of Hydrogen Chloride.*

It is shown from the results of one set of experiments at a temperature of  $30^\circ$  that the increment in the value of the rate of change per unit of chloride per unit of iodide is constant when a cubic centimetre contains 14  $HI$ , and quantities of chloride ranging from 70 to 280  $HCl$ , the value of the increment being  $16.8 \times 10^{-6}$ . An examination of four sets of observations made in the course of the experiments

\* As in our former papers, we use  $H$  to represent one-millionth of a gram of hydrogen, and other symbols italicized for the corresponding proportions of other elements. Thus  $H^2SO^4$  is 98 millionths of a gram of hydrogen sulphate.

upon temperature, and of the experiments recorded in a previous paper, made to determine the law of variation of the rate of change with iodide, shows that when the ratio of the numbers of  $HCl$  and  $HI$  is higher than it is in the set quoted above, the value of the increment per unit of chloride per unit of iodide falls to  $16.2 \times 10^{-6}$ . This lower value is the same for values of the number of  $HCl$ , ranging from 190 to 547, and for values of the ratio of the numbers of  $HCl$  and  $HI$ , ranging from 20 to 210.

The formulæ for  $\alpha$  in these two cases in a solution containing in 1 c.c. 1  $HI$  and  $c$   $HCl$  are at a temperature of  $30^\circ$ ,

$$(1.) \alpha = (1345 + 16.8c)10^{-6},$$

$$(2.) \alpha = (1230 + 16.2c)10^{-6}.$$

#### *Variation of Iodide.*

It was shown in a previous paper that when quantities of iodide are introduced into the medium sufficiently small in amount, in comparison with other substances not participating directly in the reaction, the rate of chemical change varied directly with the amount of iodide. It was conjectured that when the amount of iodide is large enough to modify considerably the character of the medium, it would have the same kind of effect upon the reaction as hydrogen sulphate and hydrogen chloride. It is now shown that the increment of the rate per unit of hydrogen iodide per unit of iodide is  $19.4 \times 10^{-6}$  at a temperature of  $30^\circ$ , the rate with 1  $HI$  at the same temperature being  $1210 \times 10^{-6}$ . The formula for the rate with  $iHI$  is

$$\alpha = i \{ 1210 + 19.4(i-1) \} 10^{-6}.$$

It will be observed that the actual rate with unit of hydrogen iodide in a medium consisting of water and hydrogen iodide is approximately the same as the theoretical rate with unit of iodide in a medium consisting of water, hydrogen iodide, and either hydrogen sulphate or hydrogen chloride when the ratio of the numbers of  $H^2SO^4$  and  $HI$  and of the numbers of  $HCl$  and  $HI$  exceeds 20. It has been shown above that this rate is  $1230 \times 10^{-6}$ .

#### *Variation of Sodium Hydrogen Carbonate.*

In a medium consisting mainly of water and sodium hydrogen carbonate the increment of the rate per unit of  $NaHCO^3$  per unit of iodide is  $40 \times 10^{-6}$  at a temperature of  $15^\circ$ , the formula for the rate with 8.59  $NaI$  and quantities of the carbonate ranging from 25  $NaHCO^3$  to 227  $NaHCO^3$ , being at this temperature

$$\alpha = 8.59 \{ 1.02 + 40b \} 10^{-6}.$$

The rate with 1 *NaI* and *bNaHCO*<sup>3</sup> is

$$\alpha = (155 + 40b)10^{-6},$$

so that the theoretical rate with 1 *NaI* only, in this medium, is  $155 \times 10^{-6}$  at a temperature of 15°. At the same temperature the actual rate with 1 *HI* in a medium consisting of water and hydrogen iodide is  $356 \times 10^{-6}$ , a rate more than twice as great as the corresponding rate with the neutral iodide. The increment of the rate per unit of carbonate per unit of iodide is more than four times the highest increment per unit of sulphate per unit of iodide at the temperature of 15°.

*Variation of Potassium Iodide, Sodium Iodide, and Sodium Chloride, in a medium consisting mainly of Water and Sodium Hydrogen Carbonate.*

In this medium at a temperature of 15°, the increments of the rate per unit of these substances per unit of iodide are (1) for the range 10 *KI* to 30 *KI*,  $4.15 \times 10^{-6}$ ; (2) for the range 10 *NaI* to 30 *NaI*,  $4.84 \times 10^{-6}$ ; (3) for the range 1 *NaCl* to 14 *NaCl*,  $27.7 \times 10^{-6}$ . In this medium sodium chloride has a considerable effect on the rate, but in a medium consisting of water and hydrogen chloride its effect is almost nil.

*General Conclusion as to the effect of the Medium upon the reaction.*

Each constituent of the medium produces an effect on the rate of change of unit peroxide and unit iodide, proportional to the mass, and varying with the nature of the constituent. The increment of this rate per unit mass of each constituent is constant so long as the quantity of the predominant constituent present in the medium is sufficiently large, in comparison with the other constituents of the medium, to render the media in successive experiments practically homogeneous. For example, when the ratio of the numbers of *H*<sup>2</sup>*SO*<sup>4</sup> and *HI* in the medium exceeds 20 the formula for the rate at a given temperature is

$$\alpha = i\{a + b(i-1) + ds\},$$

$\alpha$  being the theoretical rate with unit of *HI*,  $b$  the increment per unit of hydrogen iodide per unit of iodide, and  $d$  the increment per unit of hydrogen sulphate per unit of iodide. If the ratio falls below 20 the formula is

$$\alpha = i\{a + b'(i-1) + d's\},$$

in which  $b'$  and  $d'$  depend upon the relative masses of sulphate and iodide present in the medium.

*Variation of Temperature.*

The discussion of the numerous experiments made at temperatures ranging from  $0^{\circ}$  to  $50^{\circ}$ , in media in which the quantities of iodide range from 3.64 *HI*, to 23 *HI*, the quantities of hydrogen sulphate from 45  $H^2SO^4$  to 468  $H^2SO^4$ , and the quantities of hydrogen chloride from 70 *HCl* to 547 *HCl*, leads to the following law of connexion between chemical change and temperature.

If  $\alpha_1$  is the rate of chemical change at a temperature  $t_1^{\circ}$  in a homogeneous medium consisting of given constituents per unit volume, and  $\alpha_2$  is the rate at a temperature  $t_2^{\circ}$  in the same medium, the ratio of  $\alpha_1$  to  $\alpha_2$  is  $\{(273+t_1)/(273+t_2)\}^m$ .  $m$  being a constant depending upon the character of the constituents of the medium. When the temperatures are measured from the absolute zero  $-273^{\circ}$ , and are denoted by  $T_1$ ,  $T_2$ , the formula assumes the simpler form,

$$\alpha_1/\alpha_2 = (T_1/T_2)^m.$$

The constancy of the value of  $m$  for a particular medium is secured when the quantity of the predominant constituent of the medium is sufficiently large in comparison with the quantities of the other constituents to make the medium practically homogeneous. When this is not the case  $m$  has some value intermediate to the values which it has when one or other of the constituents is sufficiently predominant to secure a constant value.

In media in which hydrogen sulphate is sufficiently predominant, the value of  $m$  is 20.38; similarly for hydrogen chloride the value of  $m$  is 21.17. When the medium consists of water and hydrogen iodide, the value of  $m$  is 24.1. The introduction of sodium sulphate in large quantity into a medium otherwise consisting mainly of hydrogen sulphate reduces the value of  $m$  from 20.38 to 18.1. In a medium in which the main ingredient is sodium hydrogen carbonate, the value of  $m$  is approximately 10.

A further confirmation of the law of connexion between chemical change and temperature is obtained from the discussion of experiments on the rate of change of hydrogen chlorate and potassium iodide made by W. H. Pendlebury and M. Seward. The value of  $m$  is in the case of this chemical change 40.5.

It follows from the law enunciated above that at the temperature of absolute zero no chemical change can take place.

If the smallest value of  $m$ , viz., 10, is taken, a chemical change which is completed in one minute at a temperature zero, would require for its completion at a temperature of  $-200^{\circ}$  a little more than a year. If 20 is taken as the value of  $m$ , the minute would be increased to more than half a million of years by the same reduction of temperature.

The law enunciated above may also be stated in the following form.

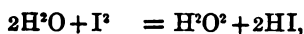
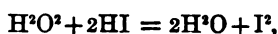
The increment of each unit of chemical change due to a rise of temperature varies as the increment of each unit of absolute temperature.

This law is expressed by the formula

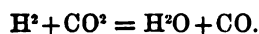
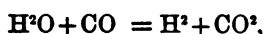
$$D\alpha/\alpha = mDT/T.$$

#### *Chemical Equilibrium.*

A case of equilibrium between the reactions



leads to a discussion of the general equations of chemical equilibrium which is given in an appendix to the paper. These equations are employed to interpret the results of experiments published by Dr. Gladstone in the Transactions of the Royal Society ('Phil. Trans.,' vol. 145). They had been previously applied to the case of chemical equilibrium investigated by Professor Dixon in a paper published in vol. 175 of the Transactions of the Royal Society, the reactions in that case being



The following Papers were also read :—

- I. "On the new Gas obtained from Uraninite. Second Note."  
By J. NORMAN LOCKYER, C.B., F.R.S. Received May 8, 1895.

Since my communication on the gas obtained from uraninite (bröggerite) was sent into the Society, on the 25th ultimo, I have been employing the method I there referred to in several directions, among them to determine whether the spectrum of the gas indicates a simple or a complex origin.

I was led to make this special inquiry on account of the difference in the frequency of the appearance of  $D_2$  and the other lines to which I referred in the previous communication in the solar chromosphere. For instance, if we take the lines  $D_2$ , 4471, and 4302, the frequencies are as follows, according to Young\* :—

\* See 'Solar Physics,' Lockyer, p. 612.

|                |               |
|----------------|---------------|
| D <sub>3</sub> | 100 (maximum) |
| 4471           | 100        „  |
| 4302           | 3             |

Hence we might be justified in supposing that D<sub>3</sub> and 4471 are produced by the same gas, while it was probable that 4302 owed its origin to a different one.

But further experiment has given me one case in which D<sub>3</sub> shows bright, while 4471 is entirely absent. I may now add that an equally important line to 4471, one at 4026·5, with the dispersion employed, appears in the spectrum of bröggerite, and both these lines are wide and fluffy, like the lines of hydrogen, and are apparently reversed.

The line 4026·5 has not been recorded by Young, though, as I have stated, the frequency of appearances of 4471 represents the maximum; still, while this is so, the intensity of both these lines in the spectra of the hottest stars is not surpassed even by those of hydrogen. Hence, opinion as to their representing the same gas must be suspended.

Further, I have photographed a line at 4388, apparently coincident with another important line in the same stars. Whether coming from one source or two, in these three lines seen along with D<sub>3</sub> in the gas obtained by me from bröggerite, we have, it would seem, run home the most important lines in the spectra of stars of Group III, in which stars alone we find D<sub>3</sub> reversed. Should these results be confirmed, the importance of the gas or gases they represent, at a certain stage of the evolution of suns and planets, will be gathered from the photograph of Bellatrix, accompanying the following Third Note.

Another case is afforded by a line at  $\lambda$  667; this is associated with D<sub>3</sub> in bröggerite and clèveite, but the yellow line has been seen in monazite *without*  $\lambda$  667. It is almost certain, then, that these two lines represent two gases. Certainty cannot be arrived at till a larger quantity of gas has been obtained.

Again the red line at  $\lambda$  6575, close to C, referred to in my previous communication, is seen both in gummite and bröggerite, but in one case (gummite) it is seen without D<sub>3</sub>, and in the other with it; in one case (bröggerite) without  $\lambda$  614, and in the other with it. The above conclusions hold here also.

This line  $\lambda$  614, possibly coincident with a chromospheric line, has been recorded in gummite and bröggerite. It has been seen *with* D<sub>3</sub> (in bröggerite) and *without* it (in gummite).

I have said enough to indicate that the preliminary reconnaissance suggests that the gas obtained from bröggerite, by my method, is one of complex origin.

I now proceed to show that the same conclusion holds good for the gases obtained by Professors Ramsay and Clève from clèveite.

For this purpose, as the final measures of the lines of the gas as obtained from clèveite by Professors Ramsay and Clève have not yet been published, I take those given by Crookes,\* and Clève,† as observed by Thalén. These are as follows, omitting the yellow line :—

| Crookes. | Thalén. |
|----------|---------|
|          | 6677    |
| 568·05   |         |
| 566·41   |         |
| 516·12   |         |
|          | 5048    |
|          | 5016    |
| 500·81   |         |
|          | 4922    |
| 480·63   |         |
|          | 4713·5  |

The most definite and striking result so far obtained is that in the spectra of the minerals giving the yellow line I have so far examined, I have never once seen the lines recorded by Crookes and Thalén in the blue. This demonstrates that the gas obtained from certain specimens of clèveite by chemical methods is vastly different from that obtained by my method from certain specimens of bröggerite, and since, from the point of view of the blue lines, the spectrum of the gas obtained from clèveite is more complex than that of bröggerite, the gas itself cannot be more simple.

Even the blue lines themselves, instead of appearing *en bloc*, vary enormously in the sun, the appearances being—

$$4922 \text{ (} 4921\cdot3 \text{)} = 30 \text{ times.}$$

$$4713 \text{ (} 4712\cdot5 \text{)} = \text{twice.}$$

These are not the only facts which can be adduced to suggest that the gas from clèveite is as complex as that from bröggerite, but while, on the one hand, the simple nature of the gases obtained by Professors Ramsay and Clève, and by myself, must be given up, reasoning on spectroscopic lines, the observations I have already made on several minerals indicate that the gases composing the mixtures are by no means the only ones we may hope to obtain.

This part of the inquiry will be more specially considered in a subsequent communication.

I may remark in conclusion, that in this preliminary inquiry no

\* 'Nature,' vol. 51, p. 544.

† 'Comptes Rendus,' April 16, p. 835.

attempt has been made to separate the possibly new gases from the known ones which come over with them; hence, the lines are in some cases very dim, and the application of high dispersion is impossible. The wave-lengths therefore, especially in the visible spectrum, are approximations only; but the view that we are really dealing with gases operative in the solar atmosphere, like the helium which produces  $D_3$ , is strengthened by the fact that of the sixty lines so far recorded as new in the various minerals examined, about half occur near the wave-lengths assigned to chromospheric lines in Young's table. I am aware that most of the chromospheric lines have been recently referred to as due to iron,\* but I believe this result does not depend upon direct comparisons, and it is entirely opposed to the conclusions to be drawn from the work of the Italian observers, as well as from my own.

## II. "On the new Gas obtained from Uraninite. Third Note."

By J. NORMAN LOCKYER, C.B., F.R.S. Received May 9, 1895.

In my preliminary note communicated to the Royal Society on the 25th ult., I gave the wave-lengths of the lines which had been observed both at reduced and at atmospheric pressure in the gas (or gases) produced by the method to which I then referred of heating the uraninite mineral (bröggerite) *in vacuo*.

As a short title in future, I shall term this the distillation method.

Since then the various photographs obtained have been reduced, and the wave-lengths of the lines in the structure spectrum of hydrogen observed beyond the region mapped by Hasselberg. I have further observed the spectra of other minerals besides uraninite for the purpose of determining whether any of them gave lines indicating the presence of the gas in uraninite or of other gases.

I now give a table of the lines so far measured in the spectra of eighteen minerals between  $\lambda\lambda$  3889 and 4580 (Rowland), the region in which, with the plates employed, the photographic action is most intense.

On this table I may remark that of the lines given in my paper of April 25, the final discussion has shown that the following lines are hydrogen structure-lines in the region beyond that mapped by Hasselberg,  $\lambda\lambda$  4479, 4196, 4156, and 4152.5. The line 4368 is also omitted from this list as it has not been finally determined whether it coincides with a line of oxygen.

In the table, besides the wave-lengths on Ångström's and Rowland's scale, I give lines which have been observed in the sun's

\* Scheiner's 'Astronomical Spectroscopy,' Frost's translation, p. 184.

Table.

Lines photographed in the Spectra of Gases obtained from various Minerals experimented upon up to May 6.

| Wave-length. |           | Chromospheric lines.<br>(Ångström's scale.) | Eclipse lines (1893).<br>(Rowland's scale.) | Orion star lines.<br>(Rowland's scale.) | Remarks. |
|--------------|-----------|---------------------------------------------|---------------------------------------------|-----------------------------------------|----------|
| Rowland.     | Ångström. |                                             |                                             |                                         |          |
| 3889·0       | 3888·5    | 3888·73 H                                   | 3889·1 (9)                                  | (Probable.)*                            | U        |
| 3947·0       | 3946·5    | 3945·2 H                                    | 3946·0 (2)                                  | —                                       | U        |
| 3982·0       | 3981·5    | —                                           | 3982·0 (2)                                  | —                                       |          |
| 4026·5       | 4025·9    | —                                           | 4026·5 (6)                                  | 4026·5 (6)                              | U        |
| 4142·0       | 4141·3    | —                                           | —                                           | —                                       |          |
| 4145·0       | 4144·3    | —                                           | 4144·0 (3)                                  | 4144·0 (5)                              |          |
| 4177·0       | 4176·3    | 4178·8                                      | 4177·8 (2)                                  | 4178·0 (4)                              |          |
| 4182·0       | 4181·3    | —                                           | —                                           | —                                       |          |
| 4338·0       | 4337·3    | 4338·0                                      | *                                           | 4338·0 (2)                              |          |
| 4347·0       | 4346·3    | —                                           | —                                           | 4346·0 (2)                              |          |
| 4390·0       | 4389·3    | 4388·5                                      | 4390·0 (1)                                  | 4389·0 (5)                              |          |
| 4398·0       | 4397·3    | 4398·5                                      | 4398·7 (2)                                  | —                                       |          |
| 4453·0       | 4452·3    | —                                           | 4454·0 (1)                                  | —                                       |          |
| 4471·0       | 4470·3    | 4471·2                                      | 4471·8 (10)                                 | 4471·8 (6)                              | U        |
| 4515·0       | 4514·3    | 4514·0                                      | 4514·5 (2)                                  | —                                       |          |
| 4522·0       | 4521·3    | 4522·0                                      | 4522·9 (2)                                  | —                                       |          |
| 4580·0       | 4579·3    | —                                           | —                                           | —                                       |          |

U = lines noted frequently in the spectra of bröggerite.

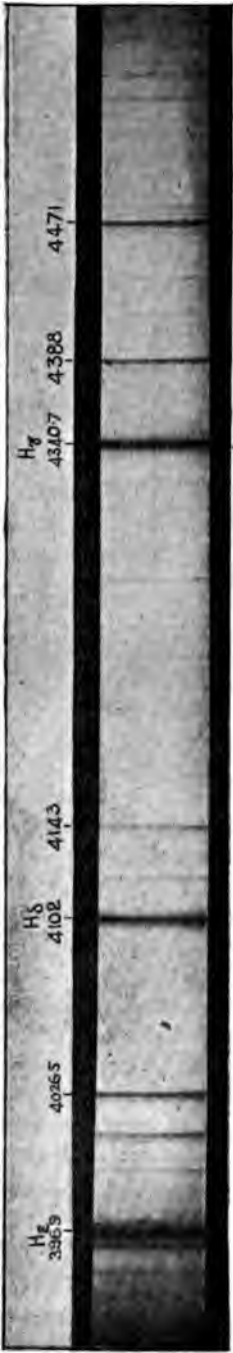
H = lines photographed by Hale.

romosphere and chronicled by Young; those photographed during  
e eclipse of 1893 with a 6-in. prismatic camera by Mr. Fowler,  
d those photographed with the same instrument at Kensington in  
e stars of Group III of my classification in the constellation of  
on.

his table carries the matter of the relation of the new gases to  
r and stellar phenomena much further than I ventured to suggest  
y second note. We appear to be in presence of the *vera causa*,  
f two or three, but of many of the lines which so far have been  
ed as "unknown" by students both of solar and stellar chem-  
and, if this be confirmed, we are evidently in the presence of a  
rder of gases of the highest importance to celestial chemistry,  
h perhaps they may be of small practical value to chemists,  
e their compounds and associated elements are for the most  
dden deep in the earth's interior.

facts that all the old terrestrial gases, with the exception of  
m, are spectroscopically invisible in the sun and stars—though  
ubtless exist there—and that these new gases, scarcely yet

\* The broad hydrogen line H $\gamma$  extends over these positions.



The spectrum of Bellatrix, showing the lines at 40265, 4143, 4388, and 4471, which have been photographed in the spectra of gases obtained from minerals, reversed in the spectrum of that star.

glimpsed, have already in all probability supplied us with many points of contact between our own planet and the hottest part of our central luminary that we can get at, and stars like Bellatrix, are full of hope for the future, not only in relation to the possibility of more closely correlating celestial and terrestrial phenomena, but in indicating that terrestrial chemistry, founded on low density surface products in which non-solar gases largely enter, is capable of almost infinite expansion when the actions and reactions of the new order of gases, almost, it may be said, of paramount importance in certain stages of stellar evolution, shall have been completely studied. With regard to the differences indicated between the results of the chromospheric and eclipse observations in the above table, it may be useful to remark that Professor Young's "frequencies," invaluable though they are, must necessarily be of less importance from the present point of view than the eclipse observations obtained, it may almost be said, at the same instant of time. There may be, and doubtless are, two perfectly distinct causes for the appearance of the so-called chromospheric lines. First, the tranquil condition of the lower strata of the sun's atmosphere, which gives us the pure spectrum produced at a constant—and the highest that we know of in the sun—temperature. Secondly, the disturbed condition which fills the spectrum with lines of a so-called prominence. Formerly it was universally imagined that the prominences were shot up from below, and, in that case, the lines added would indicate a temperature *higher* than the normal. But I have sent many papers in to the Society indicating the many arguments against this view,\* and to me at the present time this view is almost unthinkable. If these disturbance lines are produced from above, they may represent the effect of many stages of *lower* temperature. Hence a list of chromospheric lines loses most of its value, unless the conditions of each observation are stated, and the phenomena appearing at the same place at the same instant of time are recorded.

Now this same place and same time condition is perfectly met by eclipse photographs, and hence I attach a great value to them; but the comparison between such eclipse observations and the spectra of certain stars indicates that the latter afford the best criteria of all.

\* They are set out at length in the 'Chemistry of the Sun,' which I published in 1887.

III. "Action du Fluor sur l'Argon." By M. HENRI MOISSAN.  
Communicated by Professor RAMSAY, F.R.S. Received  
May 9, 1895.

La belle découverte de l'argon par Lord Rayleigh et M. Ramsay vient de nous démontrer que les corps gazeux, comme les corps solides que nous rencontrons dans la nature, peuvent renfermer en très petites quantités des éléments ignorés jusqu'ici. Ce gaz argon présente de plus un caractère tout spécial, car son activité chimique est à peu près nulle. Ce sont ses propriétés physiques qui en ont permis la recherche, la préparation et l'étude.

M. Ramsay ayant eu la complaisance de me confier 100 cm. de cet argon pour essayer de le combiner au fluor, j'ai pu faire sur ce nouveau gaz quelques essais que je rapporterai brièvement.

Depuis plusieurs années j'ai eu l'occasion d'étudier plusieurs corps simples qui dans des conditions de température déterminée se combinent à l'azote avec une grande énergie. Tels sont, en particulier, le bore et le titane.

J'ai chauffé du titane aussi pur que possible dans une atmosphère d'argon à la température de ramollissement du verre ordinaire, et après 30 minutes de chauffe il n'y a pas eu de diminution de volume, par conséquent, pas de combinaison vraisemblable. Le titane n'avait pas changé d'aspect.

Le bore pur préparé par le magnésium chauffé dans une cloche courbe de verre de Bohême ne s'est pas davantage combiné à l'argon, tandis que dans les mêmes conditions, il s'unit à l'azote pour donner l'azoture de bore solide.

Cet argon chauffé en présence de lithium\* ne diminue pas de volume, et l'on sait que dans ces conditions, l'azote fournit rapidement un azoture solide de lithium ainsi que M. Ouvrard l'a démontré.†

La curieuse décomposition de la clèveite par l'acide sulfurique m'a amené à rechercher si l'uranium n'aurait pas d'action sur l'argon. Un volume d'argon mesuré sur la cuve à mercure a été chauffé dans une petite cloche courbe en verre ordinaire en présence de plusieurs fragments d'uranium. Après vingt minutes de chauffe, le volume gazeux n'a pas été modifié. Cet uranium avait été préparé au four électrique et renfermait 3.9 pour cent de carbone.

Pour étudier l'action du fluor sur ce nouveau corps simple, je me suis servi de l'appareil suivant :—

\* Je dois ce lithium métallique à l'obligeance de M. Guntz. Ce métal a été préparé par le procédé d'électrolyse indiqué par ce savant.

† Ouvrard, "Sur un Azoture de Lithium." "Comptes Rendus de l'Académie des Sciences," t. 114, p. 120.

Un cylindre de platine de 10 cm. de longueur et de 2 cm. de diamètre est fermé par deux montures métalliques portant des disques de fluorine transparente, à faces parallèles. Latéralement deux petits tubes en platine peuvent amener l'un du fluor, l'autre de l'argon. Ces deux tubes de platine se trouvent en face l'un de l'autre. Un troisième tube placé à l'extrémité du cylindre laisse écouler le mélange gazeux. De petits robinets métalliques permettent de fermer l'appareil, sans cependant pouvoir exercer une pression un peu forte. Enfin une tige métallique isolée par la lame transparente de fluorine qu'elle traverse, permet de faire jaillir à l'intérieur, l'étincelle d'une bobine d'induction.

Les montures métalliques qui contiennent les disques de fluorine transparente ainsi que les extrémités des tubes sont serrées au moyen d'écrous qui écrasent une petite couronne de plomb placée sur le rebord des deux tubes à réunir.

Ce tube séché avec soin a été placé à la suite de l'appareil\* qui m'a servi à préparer le fluor pur et qui a été décrit aux 'Annales de Chimie et de Physique,' 6e série, Tome xxiii.

On a balayé tout l'air qui se trouvait dans le cylindre de platine en y faisant passer trois ou quatre litres de fluor, et l'on fait arriver par l'autre petit tube de platine le gaz argon déplacé lentement par du mercure au moyen d'une petite cloche à robinet. Lorsque le courant très lent de gaz argon est arrivé dans l'atmosphère de fluor aucun phénomène visible ne s'est produit. En tenant le tube dans les doigts on n'a ressenti aucun changement de température.

Dans ce mélange d'argon et de fluor on a fait jaillir l'étincelle électrique qui n'a pas produit de réaction sensible.

L'expérience a été recommencée deux fois de façon à faire varier la proportion d'argon, et les résultats ont été aussi négatifs.

La difficulté de manier le fluor ne nous a pas permis de reconnaître si par l'action d'une série d'étincelles il y aurait eu à la longue un changement de volume.

La conclusion que nous pouvons tirer de ces expériences est la suivante:—

À la température ordinaire ou sous l'action d'une étincelle d'induction un mélange de fluor et d'argon n'entre pas en combinaison.

\* L'appareil producteur de fluor était muni d'un petit réfrigérant et de deux tubes à fluorure de sodium.

*Presents, May 9, 1895.*

Transactions.

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- Berwickshire Naturalists' Club. History. 1893. 8vo. *Alnwick* 1894. The Club.
- Florence:—R. Istituto di Studi Superiori Pratici e di Perfezionamento. Pubblicazioni. 1888–89. 8vo. *Firenze*. The Institute.
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- Leipsic:—Königl. Sächsische Gesellschaft der Wissenschaften. Abhandlungen (Math.-phys. Classe). Bd. XXI. No. 6. 8vo. *Leipzig* 1895; Berichte (Math.-phys. Classe). 1894. No. 3. 8vo. *Leipzig* 1895. The Society.
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- Geological Society. Quarterly Journal. Vol. LI. Part 2. 8vo. *London* 1895; Geological Literature added to the Geological Society's Library during the Half-year ended December, 1894. 8vo. *London* 1895. The Society.
- Royal Horticultural Society. Journal. Vol. XVII. Parts 3, 4. 8vo. *London* 1895. The Society.
- Royal Photographic Society. Journal and Transactions. Vol. XIX. No. 8. 8vo. *London* 1895. The Society.
- Saint Bartholomew's Hospital. Reports. Vol. XXX. 8vo. *London* 1894. The Hospital.
- Society of Biblical Archæology. Proceedings. Vol. XVII. Part 3. 8vo. *London* 1895. The Society.
- University. Calendar. 1895–96. 8vo. *London* 1895. The University.
- Naples:—Accademia delle Scienze Fisiche e Matematiche. Rendiconto. Anno XXXIV. Fasc. 3. 8vo. *Napoli* 1895. The Academy.
- Prague:—Gesellschaft zur Förderung Deutscher Wissenschaft, Kunst und Literatur in Böhmen. Mittheilung. Nr. 4. 8vo. *Prag* 1895. The Society.
- Vienna:—Anthropologische Gesellschaft. Mittheilungen. Bd. XXIV. Heft 6. 8vo. *Wien* 1894. The Society.
- Kais. Akademie der Wissenschaften. Sitzungsberichte. Math.-Naturw. Classe. Bd. CIV. Abth. 2b. Heft 1–2. 8vo. *Wien* 1895. The Academy.

May 16, 1895.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "On Measurements of small Strains in the Testing of Materials and Structures." By J. A. EWING, M.A., F.R.S., Professor of Mechanism and Applied Mechanics in the University of Cambridge. Received April 24, 1895.

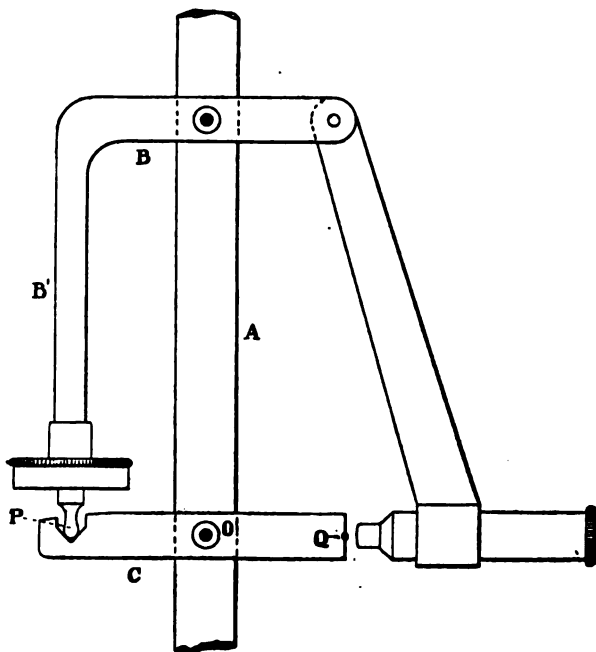
Many forms of "extensometer" have been devised for measuring the small strains of extension or compression which precede yielding in materials subjected to stress by direct pull or push. Such instruments are employed, in the testing of metals, for determining Young's modulus and for observing the behaviour of the material as the limit of elasticity is passed. Apparatus of the same general kind has also been applied to examine the strains produced in the members of bridges and other structures by the application of load to the structure as a whole; the amount of stress in any member being in that case inferred from observation of the strain. Professor Unwin, who has himself designed more than one form of extensometer, has described in his work on the 'Testing of Materials of Construction' a number of instruments of this class, and has pointed out that a first condition of accuracy is that the measurements of extension be made on both of two sides of the piece, in such a way that a mean is obtained representing the extension of a central line. In practice, a rod always bends more or less on being pulled. Even when the rod is initially straight the pull is rarely, if ever, so symmetrically applied or the elasticity so uniform as to make the extension equal at all parts of the section. Hence, to avoid errors which would be great relative to the quantity under examination, the extensometer must be arranged in such a fashion that its indications depend only on the change of distance between two points on the axis of the rod, and are independent of those inequalities which are found to exist in the strains as measured on the surface. The condition is met either by taking two separate observations of the strain on two sides of the rod and averaging the two, or by making

the extensometer itself indicate a mean strain. Bauschinger's apparatus, in which two mirrors are used to indicate separately the strains on opposite sides of the rod, is an example of the first method. Professor Unwin's extensometer, in which the rod is grasped by a pair of clips at the extremities of two parallel diameters, the change of distance between the clips being measured, is an example of the other.

The object of the present paper is to describe an instrument of the same class, but embodying some novel features, and to mention a few results that have been obtained by its means.

The instrument is self-contained, is entirely supported by the rod under test, and touches the rod only at the points where the clips are attached—a feature which is of advantage in view of the tendency the rod has to bend. The principle of the construction will be seen on referring to the diagrammatic sketch, fig. 1. To the rod A, which is

FIG. 1.



the piece under test, the two pieces B and C are attached each by a pair of set screws. Each of these pieces has, separately, one degree of freedom with respect to the rod, namely, freedom to rotate about the axis of its set screws. Hence, B has two degrees of freedom rela-

tively to C, so long as the two are not otherwise connected. But the piece B has fixed to it an arm B', which ends in a rounded point P, and this point gears in a V-slot cut transversely across the end of the piece C. The contact of P with the two sides of this slot removes the two degrees of freedom which B would otherwise have relatively to C, and makes the position of the pieces definite. Then, when the rod extends, the point P acts as a fulcrum, and the opposite end of C, namely, Q, moves down through a distance proportional to the mean displacement of the two pairs of set screws. The displacement of Q is a multiple of the strain, in the ratio of PQ to OQ. This displacement is measured by means of a microscope, which hangs from B and sights an object at Q. The strains to be observed are so small that it is easy, without making the piece C inconveniently long, to prevent its angular movement from becoming sufficient to prove troublesome or to affect the accuracy of the indications. The arm B', which carries the point P, must be so attached to B that, as regards oscillation about the set-screws of B, the two pieces B and B' move as a rigid whole. But B' may be flexible in the direction which would give P motion at right angles to the plane of the paper, and by giving it flexibility of this kind (while preserving its rigidity as regards motion in the plane of the paper) the point P may be made to gear in a hole instead of a slot in C. There is a practical advantage in doing this, for if P gears in a slot any unequal extension of the front and back of the specimen makes P work along the slot, and it is difficult to secure that no error will result in consequence of the slot not being perfectly parallel to the axes of the set-screws. This consideration has led the author to adopt the plan of putting a transverse joint between the pieces B and B', with the object of giving P freedom to adjust itself to a hole (instead of a slot) in C, by movement about this joint in a direction perpendicular to the plane of the sketch. Incidentally, such a joint has the further advantage that it allows the whole apparatus to be more conveniently and quickly applied to the rod under examination.

The displacement of Q due to the strain is measured by means of a micrometer in the eye-piece of the microscope. A micrometer scale engraved on glass is convenient for the purpose, and by estimating tenths of a division on such a scale readings are readily taken which correspond to  $\frac{1}{10000}$  of an inch of extension on the part of the specimen, with an objective which allows the whole of the elastic extension to occur without displacing Q beyond the field of view.

There is a fine screw with a divided head between B' and the point P. This serves to bring Q into a convenient position for sighting, and also to determine what is the absolute amount of extension corresponding to a division of the eye-piece scale. Should a strain occur

exceeding the range of the eye-piece scale, Q is brought back into the field of view by turning P through a distance which is observed by means of a divided head.

FIG. 2.

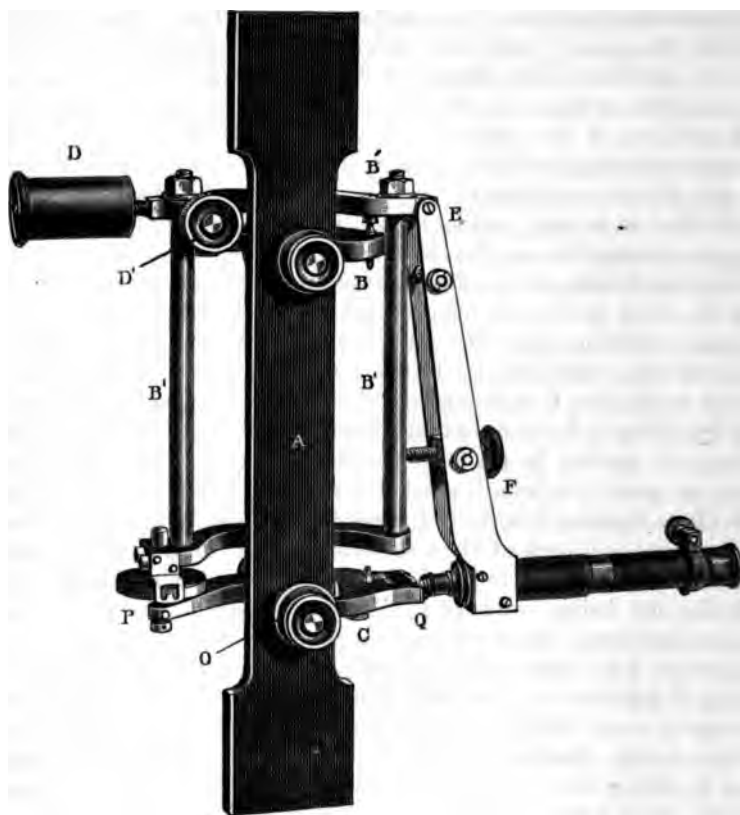


Fig. 2 is a view of the complete instrument, taken from a photograph. The clips B and C are in this instance set at 8 inches apart, and B is jointed to B' by a transverse joint, giving P freedom to accommodate itself to a hole in C. The joint between B and B' consists of two upright pins fixed in B, one of which presses up into a hole and the other into a slot in B', the line of this hole and slot being perpendicular to the axis of the set-screws by which the clip is attached to the rod under test. Hence, so far as movement about the axis of the set-screws is concerned, B and B' act as a rigid whole. This movement is prevented by the gearing of P in the hole in the lower clip C. The piece B' is a frame consisting of two

parallel steel rods united by a cross-bar at top and bottom, and carrying, besides the screw P, the microscope, which is hinged to B' about the point E vertically above Q, and is provided with a focussing screw at F. The counterpoise D, which is also attached to the piece B', serves to balance the weight of the microscope and make the pressure vertical between P and the hole into which it gears. There is a supplementary counterpoise D' for adjusting the balance about the axis of the joint between B and B'. These counterpoises are adjusted until when the heavy end (Q) of C is raised, so that P ceases to be in gear with C, P has no tendency to move in any direction. The excess of weight on the right hand side of C suffices to produce the requisite pressure at the point P. The frame BB' with the microscope may be lifted off, leaving only the two clips attached to the rod.

The object sighted is one side of a wire stretched horizontally across a hole in a plate at Q, and illuminated by a small mirror behind. The distances OP and OQ are in this instance equal, with the effect that the movement of Q is double the extension of the rod. The eye-piece scale and the length of the microscope are chosen so that the numbers read on the scale correspond to  $\frac{1}{80000}$  of an inch of extension. This adjustment is tested by turning the screw P, which has a pitch of  $\frac{1}{16}$  inch, through one revolution, and observing that the displacement of Q is 500 units of the eye-piece scale. In the instrument illustrated in fig. 2 the whole scale comprises 1,400 units, and calibration tests show that throughout the middle 1,200 of them the proportionality of the scale readings with the real movements of Q is practically perfect.

To facilitate the application of the apparatus to any rod a clamp or distance piece, H (fig. 3), is added by which the two clips, B and C, may be held at the right distance apart, and with the axes of their set screws parallel. This makes it in many cases unnecessary to prepare the rod beforehand by punching or drilling holes for points of the set screws; the clamp is readily held so that the clips stand fairly round the rod; the set screws are advanced to grip the rod, and the clamp is then removed by releasing the screws, GG. The connection of the clamp to each of the clips is by means of three points gearing with a hole, slot, and plane. The clamp is specially convenient when the strain of the specimen has been carried beyond the elastic limit, and it is desired immediately to reset the clips to the standard distance apart after the length between them has been materially changed by the permanent extension of the specimen.

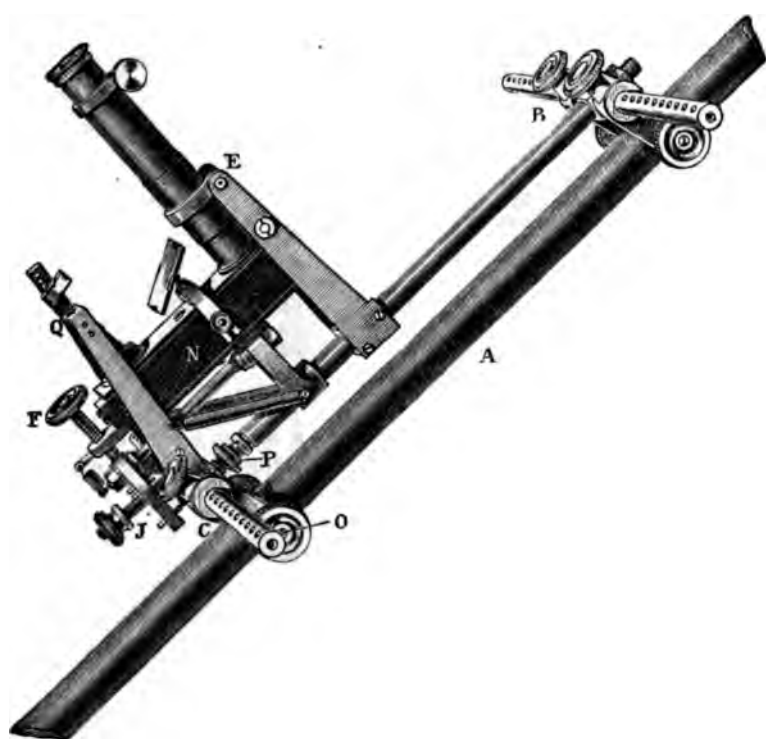
Another form of the apparatus designed for use on the members of actual structures, is shown in fig. 4. There are essentially the same parts, but the arrangement is altered in order to make all the apparatus lie on one side of the piece under examination—a desirable and

FIG. 3.



often necessary condition in the application of the instrument to such pieces as the girder flanges. The clips are (as in Professor Unwin's extensometer) adjustable to wide or narrow bars. The pieces B and C engage by means of a screw-point in a hole, the necessary pressure being supplied by springs, with the effect that the apparatus can be used in any position, horizontal, vertical, or inclined (as in the figure). The sighted wire Q, is on a prolongation of C beyond P, giving a mechanical multiplication of three to one. Between the objective and the eye-piece of the microscope is the box N, containing a series of two totally reflecting prisms, which turn the line of sight through two right angles. This arrangement keeps all parts of the instrument compactly together on one side of the bar or plate under test. A clamp is provided at J which, by the tightening up of a single nut, disengages P from its hole and fixes the two portions, B and C, in relation to one another, so that the instrument may then be handled as a rigid whole, and be readily applied to the bar which is to be tested, the axes of the clips being then parallel and at the proper distance

FIG. 4.



apart. When the instrument has been put in place, and the clip screws tightened up, the nut J is released, P comes into gear, and things are ready for the observation to be taken.

The following are examples of tests made with the author's extensometer in its laboratory form. The pieces were loaded, in tension, by means of the 50-ton Wicksteed single-lever testing machine of the Cambridge Engineering Laboratory. The machine is worked by a hydraulic intensifier, which affords a particularly convenient means of varying the load gradually without shock and without setting the weigh-beam into oscillation. Except when otherwise stated, the increments of load were made at intervals of about one minute, that interval sufficing for the adjustment of the load to a new value, and for reading the extensometer. The unit of the extensometer scale readings is in all cases  $\frac{1}{100000}$  inch.

(1.) Turned bar of mild steel. Diameter, 1.249 inches. Length under measurement, 8 inches. Temperature, 57° F. Zero of extensometer set at 400. Results of three successive loadings within the elastic limit :—

| Load in tons. | Extensometer readings. |                 |                | Differences.   |                 |                |
|---------------|------------------------|-----------------|----------------|----------------|-----------------|----------------|
|               | First loading.         | Second loading. | Third loading. | First loading. | Second loading. | Third loading. |
| 0             | 400                    | 400             | 400            |                |                 |                |
| 2½            | 461                    | 461             | 461            | 61             | 61              | 61             |
| 5             | 522                    | 522             | 522            | 61             | 61              | 61             |
| 7½            | 583                    | 583             | 583            | 61             | 61              | 61             |
| 10            | 645                    | 645             | 645            | 62             | 62              | 62             |
| 12½           | 707                    | 706             | 707            | 62             | 61              | 62             |
| 15            | 769                    | 768             | 768            | 62             | 62              | 61             |
| 17½           | 830                    | 829             | 830            | 61             | 61              | 62             |
| 20            | 892                    | 891             | 891            | 62             | 62              | 61             |
| 0             | 400                    | 400             | 400            | 492            | 491             | 491            |

Each ton corresponds in this case to a stress of 0.817 ton per square inch. Taking the whole extension due to 20 tons to be 491, or 0.00982 inch, Young's modulus is

$$E = \frac{8 \times 20 \times 0.817}{0.00982} = 13310 \text{ tons per sq. inch.}$$

(2.) Turned bar of tool steel. Diameter, 0.706 inch. Length under measurement, 9 inches. Temperature, 57° F. The bar had not been previously loaded beyond 6 tons.

| Load in tons. | Extensometer readings. | Differences. |               |
|---------------|------------------------|--------------|---------------|
|               |                        | Per ton.     | Per half-ton. |
| 0             | 100                    | —            | —             |
| 1             | 184                    | 84           | —             |
| 2             | 268                    | 84           | —             |
| 3             | 352                    | 84           | —             |
| 4             | 437                    | 85           | —             |
| 5             | 522                    | 85           | —             |
| 6             | 606                    | 85           | —             |
| 6½            | 649                    | —            | 43            |
| 7             | 691                    | 85           | 42            |
| 7½            | 733                    | —            | 42            |
| 8             | 776                    | 85           | 43            |
| 8½            | 819                    | —            | 43            |
| 9             | 862                    | 86           | 43            |
| 9½            | 904                    | —            | 42            |
| 10            | 947                    | 85           | 43            |
| 10½           | 990                    | —            | 43            |
| 11            | 1032                   | 85           | 42            |
| 11½           | 1075                   | —            | 43            |

|        | Load in<br>tons. | Extensometer<br>readings. | Differences. |               |
|--------|------------------|---------------------------|--------------|---------------|
|        |                  |                           | Per ton.     | Per half-ton. |
|        | 12               | 1119                      | 87           | 44            |
| Unload | 0                | 100½                      | —            | —             |
| Reload | 12               | 1119                      | —            | —             |
|        | 12½              | 1163                      | —            | 44            |
|        | 13               | 1206                      | 87           | 43            |
|        | 13½              | 1249                      | —            | 43            |
|        | 14               | 1292                      | 86           | 43            |

The load was then removed, observation being taken at these stages :—

| Load. | Extensometer. |
|-------|---------------|
| 10    | 950           |
| 7     | 693           |
| 5     | 523           |
| 0     | 101½          |

Here each ton of load corresponds to 2.56 tons per square inch. The mean extension up to 10 tons is 84.7 per ton, and corresponds to a Young's modulus,  $E$ , of 13600 tons per square inch. The extension per ton increases slightly, but perceptibly, with the later loads, although the almost complete absence of set after 12 tons, and even after 14, shows that there was no very clear passing of an elastic limit, even under the greatest strain of 35.8 tons per square inch. There is just a trace of hysteresis in the relation of strain to stress which shows itself in the removal of the load.

These two examples, relating as they do to cases where Hooke's law of the proportionality of strain to stress is very approximately true, may be taken to serve as tests of the sensibility and accuracy of the extensometer.

In the examples which follow, the limits were passed within which Hooke's law applies. In augmenting the stress so as to pass these limits, it very generally happens that one of the first evidences of overstrain is a time effect—a creeping up of the extensometer reading—while the load is kept constant for a few minutes. This creeping, which is a familiar phenomenon in the measurement of strains, can usually be detected a good way before the yield point is reached. It is associated with failure in proportionality of strain to stress, and also with permanent, or semi-permanent set. In certain conditions of the material as regards previous treatment, this creeping is far more marked than in other conditions. It shows itself most when the piece has, immediately before the test, been subjected to a load sufficient to cause permanent yielding to occur. But if the same piece is allowed to rest for some days, and is then re-tested, the tendency to creeping is found to have disappeared or to be much

reduced in consequence of the hardening and recovery of elasticity which the overstrained material undergoes with the mere lapse of time.

The following observations were made with a turned rod of common wrought iron, the original diameter of which was 0.697 inch (section 0.381 square inch). Length under measurement, 9 inches. Each ton of load corresponds to 2.65 tons per square inch. The rod was annealed after being turned. The first loading, after annealing, gave the following results:—

## (3A.) Common Iron, annealed.

|        | Load in tons.  | Extensometer readings. | Differences.                   |           |
|--------|----------------|------------------------|--------------------------------|-----------|
|        |                |                        | Per half-ton.                  | Per ton.  |
|        | 0              | 200                    | —                              | —         |
|        | $\frac{1}{2}$  | 250                    | 50                             | —         |
|        | 1              | 299                    | 49                             | —         |
|        | $1\frac{1}{2}$ | 447                    | 48                             | —         |
|        | 2              | 395                    | 48                             | —         |
|        | $2\frac{1}{2}$ | 441                    | 46                             | —         |
|        | 3              | 488                    | 47                             | —         |
|        | $3\frac{1}{2}$ | 536                    | 48                             | —         |
|        | 4              | 582                    | 46                             | —         |
| Unload | 0              | 201                    | —                              | (Set = 1) |
| Reload | 1              | 300                    | —                              | —         |
|        | 2              | 395                    | —                              | 95        |
|        | 3              | 490                    | —                              | 95        |
|        | 4              | 584                    | —                              | 94        |
|        | $4\frac{1}{2}$ | 631                    | 47                             | —         |
|        | 5              | 682                    | 51                             | —         |
| Unload | 0              | 208                    | —                              | (Set = 8) |
| Reload | 5              | 682                    | —                              | —         |
|        | 5.2            | 702                    | Creeping to 703 in 1 minute.   |           |
|        | 5.4            | 721                    | Creeping to 724 in 1 minute.   |           |
|        | 5.6            | 749                    | Creeping to 750 in 10 seconds. |           |
|        | 5.8            | 773                    | Creeping to 774 in 10 seconds. |           |
|        | 6.0            | 810                    | Creeping to 820 in 1 minute.   |           |

With this load of 6 tons the yield point was reached. The creeping under the load was at first slow, then gradually became accelerated, and, finally, the sighted wire of the extensometer ran quickly off the scale. At the same time the oxide formed in annealing began to come off in the way characteristic of the yield point. The scaling of the oxide began at one place in the bar and spread gradually in both directions. The load of 6 tons was kept on without altera-

tion for about three minutes while this was happening, and when it was removed there was a permanent extension of 0.10 inch on the marked length of 9 inches.

The extensometer was immediately reset to the normal length of 9 inches, an operation which occupied two or three minutes more, after which the bar was reloaded as follows :—

| (3b.) Same piece after stretching by a load of 6 tons. |                       |                     |
|--------------------------------------------------------|-----------------------|---------------------|
| Load in tons.                                          | Extensometer.         | Difference per ton. |
| 0                                                      | 200                   | —                   |
| 1                                                      | 297                   | 97                  |
| 2                                                      | 395                   | 98                  |
| 3                                                      | 497                   | 102                 |
| 4                                                      | 600 (Creeping)        |                     |
|                                                        | 601 After 1 minute.   | 104                 |
| Unload 0                                               | 202 (Creeping back)   |                     |
|                                                        | 200 After 1 minute.   |                     |
| Reload 1                                               | 297                   | 97                  |
| 2                                                      | 395                   | 98                  |
| 3                                                      | 495                   | 100                 |
| 4                                                      | 598                   | 103                 |
| 5                                                      | 705                   | —                   |
|                                                        | 707 After 20 seconds. | 109                 |
| 5½                                                     | 767                   | —                   |
|                                                        | 770 After 20 seconds. |                     |
| Unload 0                                               | 214                   | —                   |
|                                                        | 210 After 3 minutes.  |                     |
|                                                        | 208 After 45 minutes. |                     |

These figures show how widely different are the elastic qualities of an iron bar in its primitive annealed condition, and in the condition in which it is put by overstrain. In the first condition there is almost no evidence of creeping or “*elastische nachwirkung*” up to say 5 tons of load, and Hooke’s law is nearly valid. Immediately after an overstrain there are distinct evidences of creeping at lower loads, and when these lower loads are removed, there is the same kind of thing in the unloaded state, namely, an apparent set which disappears in whole or in part when time is allowed for the creeping to take effect. And further, after the overstrain there is no longer nearly so close an agreement with Hooke’s law; a given increment of load produces notably more strain at high loads than at low ones. In loading and unloading there is now much hysteresis in the relation of strain to stress.

In the overstrained state the lowest loads produce not much more strain than they did in the primitive state; the value of Young’s

modulus so far as very small strains are concerned is scarcely changed. But if the modulus be calculated by reference to the effects of higher loads, it will be much smaller in the overstrained bar. This has been noticed by Bauschinger, who has also observed the tendency, illustrated in tests to be described below, which iron and steel show to recover their elasticity with the lapse of time, after overstrain has taken place.\*

On resuming the above experiment, 45 minutes after the previous load had been removed, the influence of the overstrain was still conspicuous.

| Load in tons. | Extensometer.         | Difference per ton. |
|---------------|-----------------------|---------------------|
| 0             | 208                   | —                   |
| 1             | 304                   | 96                  |
| 2             | 402                   | 98                  |
| 3             | 501                   | 99                  |
| 4             | 602                   | 101                 |
| 5             | 708                   | (106)               |
|               | 710 After 20 seconds. | 108                 |
| 6             | 833                   | —                   |
|               | 850 After 1 minute.   | —                   |
| 0             | 244                   | —                   |
|               | 240 After 1 minute.   | —                   |

The bar was then taken out of the testing machine and left to itself for 5 days. On the fifth day the following readings were taken; they show in a striking way the effect of this interval of rest:—

(3c.) Same piece after resting for 5 days.

| Load in tons. | Extensometer. | Difference per ton. |
|---------------|---------------|---------------------|
| 0             | 200           | —                   |
| 1             | 296           | 96                  |
| 2             | 392           | 96                  |
| 3             | 490           | 98                  |
| 4             | 588           | 98                  |
| 5             | 685           | 97                  |
| 6             | 782           | 97                  |
| 0             | 200           | —                   |
| 6             | 782           | —                   |
| 0             | 200           | —                   |

\* See also a paper by the present writer ("On certain Effects of Stress," *Roy. Soc. Proc.*, No. 205, 1880), which gives instances of the rise of the yield-point with lapse of time, a phenomenon evidently having a close relation to the rise of the modulus.

The tendency to creep has now practically disappeared so far as this range of load is concerned; the strain is very nearly proportional to the stress, and there is no set. It will be remembered that 6 tons was the greatest load formerly applied, and that it brought the piece to the yield-point in the original test.

The loading was then resumed, and was carried a stage further to see how far the new yield-point would be above the old one.

| Load in tons. | Extensometer. | Difference per ton.                                                     |
|---------------|---------------|-------------------------------------------------------------------------|
| 0             | 200           | —                                                                       |
| 1             | 296           | 96                                                                      |
| 2             | 393           | 97                                                                      |
| 3             | 490           | 97                                                                      |
| 4             | 588           | 98                                                                      |
| 5             | 685           | 97                                                                      |
| 6             | 782           | 97                                                                      |
| 6½            | 832           | —                                                                       |
| 7             | 908           | Creeping, at first slow, then faster, and tending to run off the scale. |
| 0             | 600           | Showing 400 divisions, or 0.008 inch, of further permanent extension.   |

The zero of the extensometer was now brought back to 200, and the loading was immediately repeated, to see whether this small amount of further overstrain had undone, to any extent, the molecular settlement which had been going on during the 5 days of rest.

| Load in tons. | Extensometer. | Difference per ton.                    |
|---------------|---------------|----------------------------------------|
| 0             | 200           | —                                      |
| 1             | 297           | 97                                     |
| 2             | 395           | 98                                     |
| 3             | 492           | 97                                     |
| 4             | 591           | 99                                     |
| 5             | 692           | 101                                    |
| 6             | 793           | 101                                    |
| 0             | 204           | Creeping back to 202, but not further. |

It is clear from these figures that the elasticity has again been, to some extent, injured by the overstrain at the 7-ton load, small as that was; and one effect is that even a load of 6 tons now produces some persistent set.

The following are values of Young's modulus  $E$ , for this bar, calculated from the foregoing experiments (3A, 3B, and 3C).

## Primitive Annealed State (3A).

Mean extension from 1 to 4 tons = 94.5 per ton ;  
 $E = 12500$  tons per square inch.

Immediately after passing yield-point (3B), the diameter was reduced by the set to 0.693 inch, and the section to 0.377 square inch.

Extension for first ton = 97 ;  $E = 12310$ .  
 Mean extension from 0 to 2 tons = 97.5 ;  $E = 12250$ .  
 Mean extension from 0 to 4 tons = 100.2 ;  $E = 11910$ .  
 Mean extension from 0 to 5 tons = 101.4 ;  $E = 11770$ .

After elastic recovery by resting for 5 days (3c).

Mean extension from 0 to 6 tons = 97 ;  $E = 12310$ .

After a small amount of further overstrain (by applying 7 tons).

Extension for first ton = 97 ;  $E = 12310$ .  
 Mean extension from 0 to 6 tons = 99 ;  $E = 12060$ .

Similar results have been obtained in tests of other bars, of mild steel, comparatively hard steel, and Lowmoor iron. In every case, overstrain has produced a like fatigue of elasticity, and elastic recovery has followed during an interval of some days or weeks of rest.

In several examples the process of passing the yield-point has been watched in the extensometer and has been seen to take place in the same manner as in test (3A). Creeping, which has been slightly visible under lower loads, takes place at first slowly, then it gradually gets faster, though no change is made in the load. This may go on for a minute or two before the crepitation of oxide begins. The following is an instance noticed in testing a bar of mild steel. The load having been increased a step, and kept constant, there were

|     |                                       |          |
|-----|---------------------------------------|----------|
| 3   | scale-divisions of creep in the first | minute   |
| 9   | "                                     | second " |
| 320 | "                                     | third "  |

after which the index of the extensometer raced off the scale. In another case the writer noticed creeping going on quite slowly, when a sound of crepitation was heard. On looking at the bar it was seen that the crepitation had begun near one end, beyond the clips. As soon as it spread far enough to reach the clip there was, of course, a rapid movement of the index.

The following test will suffice in further illustration of the

influence of overstrain and subsequent rest. It was made on a turned bar of moderately hard, or semi-mild steel, which had not been previously loaded beyond 6 tons. The diameter was 0.705 inch, and the section 0.390 sq. inch. The length under test was 9 inches.

## (4A.) Semi-mild Steel. Primitive state.

| Load in tons. | Extensometer.                                                                      | Differences. |               |
|---------------|------------------------------------------------------------------------------------|--------------|---------------|
|               |                                                                                    | Per ton.     | Per half-ton. |
| 0             | 200                                                                                | —            | —             |
| 1             | 287                                                                                | 87           | —             |
| 2             | 373                                                                                | 86           | —             |
| 3             | 459                                                                                | 86           | —             |
| 4             | 544                                                                                | 85           | —             |
| 5             | 630                                                                                | 86           | —             |
| 0             | 200                                                                                | —            | —             |
| 5             | 630                                                                                | —            | —             |
| 5½            | 672                                                                                | —            | 42            |
| 6             | 715                                                                                | —            | 43            |
| 6½            | 758                                                                                | —            | 43            |
| 7             | 800                                                                                | —            | 42            |
| 7½            | 843                                                                                | —            | 43            |
| 8             | 885                                                                                | —            | 42            |
| 8½            | 929                                                                                | —            | 44            |
| 9             | 970                                                                                | —            | 41            |
| 9½            | 1010                                                                               | —            | 40            |
| 10            | 1053                                                                               | —            | 43            |
| 0             | 201                                                                                | —            | —             |
| 10            | 1054                                                                               | —            | —             |
| 10½           | 1099                                                                               | —            | 45            |
| 11            | Yield-point passed; permanent extension produced = 0.14 inch on the 9-inch length. |              |               |

The load was removed and the extensometer reset to 9 inches, and the following test was immediately made, beginning 10 minutes after the yielding took place:—

## (4B.) Same piece, after stretching by a load of 11 tons.

| Load in tons. | Extensometer. | Difference per half-ton. |
|---------------|---------------|--------------------------|
| 0             | 200           | —                        |
| ½             | 243           | 43                       |
| 1             | 287           | 44                       |
| 1½            | 331           | 44                       |

| Load in tons.  | Extensometer.         | Difference per half ton. |
|----------------|-----------------------|--------------------------|
| 2              | 377                   | 46                       |
| $2\frac{1}{2}$ | 422                   | 45                       |
| 3              | 469                   | 47                       |
| $3\frac{1}{2}$ | 517                   | 48                       |
| 4              | 565                   | 48                       |
| $4\frac{1}{2}$ | 613                   | 48                       |
| 5              | 662                   | 49                       |
| 0              | 200                   | 48                       |
| 5              | 661                   | —                        |
| $5\frac{1}{2}$ | 710                   | 49                       |
| 6              | 760                   | 50                       |
| $6\frac{1}{2}$ | 812                   | 52                       |
| 7              | 866                   | 54                       |
| $7\frac{1}{2}$ | 920                   | 54                       |
| 8              | 975                   | —                        |
|                | 977 After 30 seconds. |                          |
| 0              | 208                   | —                        |
|                | 203 After 2 minutes.  |                          |
| 8              | 974                   | —                        |
|                | 977 After 1 minute.   |                          |
| 0              | 210                   | —                        |
|                | 207 After 30 seconds. |                          |
|                | 202 After 3 minutes.  |                          |
|                | 200 After 20 minutes. |                          |

As in the example already given, the influence of overstrain apparent (1) by the presence of creeping or "nachwirkung," and by the progressive growth of the differences as the load increases. first ton or so produces little more strain than it did before the strain took place, but the eighth ton produces a quarter as much again.

An hour after the yielding had taken place the load was again applied in stages of 1 ton as follows, and the extensometer readings already show something of elastic recovery through rest. They are given in the following table, along with readings taken on the following day and on subsequent days. It is interesting to notice the rather slow progress of the elastic recovery from day to day. It takes place much less rapidly in this comparatively hard metal than in milder steel or in wrought iron. To facilitate comparison readings taken immediately after the overstrain are repeated here.

Successive Loadings of the same Piece, after various Intervals of Time.

| Load<br>in<br>tons. | (4a.)<br>Ten minutes<br>after overstrain. |               | (4b.)<br>One hour<br>after overstrain. |               | (4c.)<br>One day<br>after overstrain. |               | (4d.)<br>Two days<br>after overstrain. |               | (4e.)<br>Three days<br>after overstrain. |               | (4f.)<br>Five days<br>after overstrain. |               | (4g.)<br>Twenty-one days<br>after overstrain. |               |
|---------------------|-------------------------------------------|---------------|----------------------------------------|---------------|---------------------------------------|---------------|----------------------------------------|---------------|------------------------------------------|---------------|-----------------------------------------|---------------|-----------------------------------------------|---------------|
|                     | Extensometer readings.                    | Differ-ences. | Extensometer readings.                 | Differ-ences. | Extensometer readings.                | Differ-ences. | Extensometer readings.                 | Differ-ences. | Extensometer readings.                   | Differ-ences. | Extensometer readings.                  | Differ-ences. | Extensometer readings.                        | Differ-ences. |
| 0                   | 200                                       | —             | 200                                    | —             | 200                                   | —             | 200                                    | —             | 200                                      | —             | 200                                     | —             | 200                                           | —             |
| 1                   | 287                                       | 87            | 286                                    | 86            | 286                                   | 86            | 286                                    | 86            | 286                                      | 86            | 286                                     | 86            | 285                                           | 85            |
| 2                   | 377                                       | 90            | 376                                    | 89            | 373                                   | 87            | 372                                    | 86            | 372                                      | 86            | 372                                     | 86            | 371                                           | 86            |
| 3                   | 469                                       | 92            | 467                                    | 91            | 463                                   | 90            | 461                                    | 89            | 461                                      | 89            | 460                                     | 88            | 458                                           | 87            |
| 4                   | 565                                       | 96            | 562                                    | 95            | 559                                   | 96            | 556                                    | 95            | 553                                      | 92            | 550                                     | 90            | 545                                           | 87            |
| 5                   | 662                                       | 97            | 660                                    | 98            | 658                                   | 99            | 653                                    | 97            | 650                                      | 97            | 643                                     | 93            | 632                                           | 87            |
| 6                   | 760                                       | 98            | 760                                    | 100           | 758                                   | 100           | 754                                    | 101           | 750                                      | 100           | 741                                     | 98            | 720                                           | 88            |
| 7                   | 866                                       | 106           | 862                                    | 102           | 860                                   | 102           | 857                                    | 103           | 853                                      | 103           | 844                                     | 103           | 810                                           | 90            |
| 8                   | 976                                       | 110           | 969                                    | 107           | 963                                   | 103           | 960                                    | 103           | 958                                      | 105           | 950                                     | 106           | 900                                           | 90            |
| 0                   | 208<br>to<br>203                          | —             | 206<br>to<br>200                       | —             | 203<br>to<br>200                      | —             | 203<br>to<br>200                       | —             | 203<br>to<br>200                         | —             | 203<br>to<br>200                        | —             | 200 <sup>1</sup><br>to<br>200                 | —             |

These figures show that even after three weeks the elastic recovery is still somewhat incomplete. This is the more remarkable when it is borne in mind that the testing is done here with a load of 8 tons only, a considerably smaller load than that which had been applied to produce permanent set (namely 11 tons). The imperfection which the elasticity still shows after three weeks of rest would be more conspicuous if the loading were extended beyond 8 tons.

Throughout this group of tests evidences were seen of much elastic "nachwirkung." If a load exceeding, say, 4 tons, was left on for a few minutes there was continued extension, amounting sometimes to as much as five scale divisions. And when the load of 8 tons was removed there was, as the table indicates, a gradual retraction which in a few minutes destroyed the apparent set observable at the moment of removing the load. In the final observation (4H) this action had almost wholly disappeared, but even then a trace of it could be detected.

As elastic recovery goes on in the days or weeks following overstrain there is a gradual return towards Hooke's law, or as it might be described in other terms, a gradual straightening out of the stress-strain curve. So long as the recovery is incomplete there can scarcely be said to be any elastic limit, in the sense of a point below which there is strict proportionality of strain to stress.

For the Young's modulus of this bar the tests give the following values:—

(4A.) Primitive condition (before overstrain).

Section 0.390 square inch.

Mean extension from 0 to 8 tons = 85.6 divisions per ton.

$E = 13480$  tons per square inch.

(4B.) Immediately after stretching past the yield-point by 11 tons. Section 0.385 square inch.

Extension for the first ton = 87;  $E = 13440$ .

Mean extension from 0 to 8 tons = 97 per ton;  $E = 12060$ .

(4G.) After 5 days' rest.

Extension for the first 2 tons = 86 per ton;  $E = 13600$ .

Mean extension from 0 to 8 tons = 93.7 per ton;  $E = 12480$ .

(4H.) After 21 days' rest.

Extension for the first 2 tons = 85.5 per ton;  $E = 13670$ .

Mean extension from 0 to 8 tons = 87.5 per ton;  $E = 13370$ .

The behaviour of iron or steel when "fatigued" by overstrain, and before recovery of elasticity has taken place through prolonged rest,

much resembles that of a brass rod in its normal state. The following is a test of a turned rod of rolled brass with a section of 0.386 square inch. The length under measurement was 9 inches.

| Load.          | Extensometer. | Difference per half-ton. |
|----------------|---------------|--------------------------|
| 0              | 200           | —                        |
| $\frac{1}{2}$  | 281           | 81                       |
| 1              | 362           | 81                       |
| $1\frac{1}{2}$ | 443           | 81                       |
| 2              | 525           | 82                       |
| $2\frac{1}{2}$ | 607           | 82                       |
| 3              | 690           | 83                       |
| $3\frac{1}{2}$ | 772           | 82                       |
| 4              | 856           | 84                       |
| $4\frac{1}{2}$ | 941           | 85                       |
| 0              | 200           | —                        |
| $4\frac{1}{2}$ | 942           | —                        |
| 5              | 1031          | 89                       |
| 0              | 202           | —                        |
| 5              | 1036 to 1037  | —                        |
| $5\frac{1}{2}$ | 1129 to 1130  | 93                       |
| 0              | 212 to 210    | —                        |

Under the higher loads there was a distinct creeping, with a corresponding creeping back after the load was removed.

Taking 163 divisions per ton as the extension under small loads, the value of E is 7160 tons per square inch.

The last test to be cited is that of a "steel" casting of the specially pure kind now extensively used to form the cores of dynamo magnets. The rod was turned from a steel casting by the makers (Messrs. Edgar Allen and Co.), and had been submitted to the writer for a magnetic test, which had shown it to have exceptionally high permeability under strong magnetic forces. It consisted of nearly pure iron. The diameter was 0.753 inch.

| Load.          | Extensometer. | Difference per half-ton. | Set.                       |
|----------------|---------------|--------------------------|----------------------------|
| 0              | 200           | —                        | —                          |
| $\frac{1}{2}$  | 237           | 37                       | —                          |
| 1              | 273           | 36                       | —                          |
| $1\frac{1}{2}$ | 310           | 37                       | —                          |
| 2              | 347           | 37                       | —                          |
| $2\frac{1}{2}$ | 384           | 37                       | —                          |
| 3              | 423           | 39                       | —                          |
| 0              | 204           | —                        | 4 (apparently persistent). |

| Load.          | Extensometer.                      | Difference per<br>half-ton. | Set. |
|----------------|------------------------------------|-----------------------------|------|
| 3              | 424                                | —                           | —    |
| $3\frac{1}{2}$ | 468 to 470                         | 44 to 46                    | —    |
| 4              | 528 to 540                         | 58 to 70                    | —    |
| 0              | 249                                | —                           | 49   |
| 4              | 545 to 550                         | —                           | —    |
| $4\frac{1}{2}$ | 670                                | —                           | —    |
|                | 715 after half a minute.           |                             |      |
|                | 758 after two minutes.             |                             |      |
|                | 785 after nine minutes.            |                             |      |
| 0              | 447                                | —                           | 247  |
| $4\frac{1}{2}$ | 798                                | —                           | —    |
| 5              | 1200 running slowly off the scale. |                             |      |

The mean extension from 0 to  $2\frac{1}{2}$  tons is 73.6 per ton, which makes  $E = 13740$  tons per square inch.

II. "The Electrical Measurement of Starlight. Observations made at the Observatory of Daramona House, co. Westmeath, in April, 1895. Preliminary Report." By G. M. MINCHIN, M.A. Communicated by Professor FITZGERALD, F.R.S. Received April 29, 1895.

The method employed in these experiments for measuring the intensity of the light which reaches the earth from the stars and planets consists in the determination of the electromotive force generated by such light in certain photo-electric cells, the square of this electromotive force being proportional to the energy of the incident light.

It will, then, be well to describe first the nature and construction of these cells.

#### *The Photo-electric Cells.*

In these cells the surface on which the incident light is received is formed by depositing a thin layer of selenium on a surface of clean aluminium, and immersing the sensitive layer in a glass cell filled with cenanthol.

The mode of formation of the sensitive surface is as follows:—

FIG. 1.



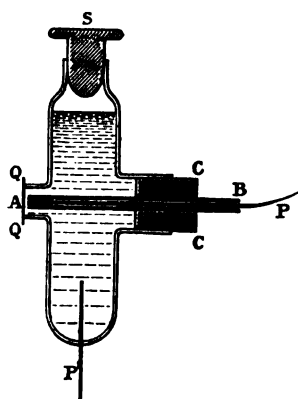
Take a tube, AB, of soft glass, the diameter of the bore of which is 1 mm., or smaller if desired; take a short length, AL, of aluminium wire, which nearly fits the tube, and to one end, L, of this piece of aluminium attach a platinum wire, LP, which emerges from the end, B, of the glass tube, the contact at L being made by boring a fine hole through the aluminium and pinching the two metals together; then, in the flame of a Bunsen burner or a blow-pipe, melt the glass well round the aluminium, until the glass fits round the aluminium as tightly as possible. The contact of the glass and the aluminium should be perfect, or, at least, liquid-tight, and, unfortunately, it has been found hitherto impossible to realise this condition. If this condition could be attained, the photo-electric cells would remain constant in their action for a very long time, if not, indeed, permanently. At present, owing to this want of liquid-tightness, about four weeks seems to be the limit of constancy. (There is also another condition essential to constancy, which will be presently mentioned.)

The next step is to grind the end A of the tube on fine sand or emery-paper until a flat surface is formed by the end of the aluminium wire and the wall of the glass tube, the end of the aluminium wire being then scraped clean.

Now place the tube AB, with the end A uppermost, between two nearly vertical plates of asbestos, the end A just appearing beyond the edges of the plates of asbestos; on the middle of the aluminium wire at A place a very small piece of selenium (about the size of a very small pin-head); heat the asbestos by means of a spirit lamp or a Bunsen flame until the selenium melts on the end A of the tube. Care must be taken to keep the flame away from the selenium, so that the latter melts in virtue of the heat of the aluminium wire. The selenium now lies as a very black little liquid globule on the end of the tube, and it must be spread uniformly over the end of the tube by means of a heated glass rod. The layer of selenium should not be a thick one. The flame being removed, allow the selenium to cool into a hard black layer. When it reaches this condition, apply the heat again, as before, until the black surface changes into one with a uniform brownish-grey colour, the heat being continued after this with great care until the selenium is on the point of melting again into a black liquid. On the first sign of this latter change, instantly remove the heat and blow over the surface of the selenium. This will at once stop the tendency to melt, and the surface will then be in its most sensitive state. There should be no glossy streaks on the surface; if there are, it must be heated over again and the whole process repeated. Screen the tube from light and allow it to cool for a few minutes; it will then be ready to put into the cell with cesanthol.

The œnanthol cell is a small glass tube, represented in fig. 2. It consists of a glass tube about 3 cm. long, and nearly a centimetre in diameter, with two short glass tubes fitted into it on opposite sides; one of these is ground flat, and has a thin quartz window, QQ, cemented to it with gelatine and acetic acid, or glue and glycerine, or any cement that will withstand the action of œnanthol; the other is tightly closed by a cork, CC, through which passes the glass tube, AB, which contains the aluminium and platinum wires above

FIG. 2.



described. The cell is closed at one end by a ground glass stopper, S, and through the other end passes a platinum wire, P', sealed in. The two poles of the cell are the platinum wires P and P'.

The light of a star is destined to shine through the quartz window QQ on the centre of the sensitive surface A, which is placed in focus of a telescope, or rather a little behind this focus, so that the light of the star may cover the whole of the selenium area.

The covering of the whole area A by the light is essential, for the following reason:—

The seat of the electromotive force is the surface of contact of the liquid and the selenium, the selenium receiving a positive and the liquid a negative charge. If, now, P is connected with one pole of an electrometer and P' with the other, and if there is any portion of the selenium surface which is not exposed to the light (and consequently not the seat of an E.M.F.), this inert portion will act simply as a conductor conveying a portion of the positive charge to the wrong pole of the electrometer, and thus giving a diminished effect.

The truth of this is easily verified with any kind of photo-electric cell, *e.g.*, one formed of a sensitive tinfoil surface divided into two

portions which can be metallically joined together outside the cell or kept separate. If, while the portions are joined, one is exposed to, and the other screened from, incident light, the E.M.F. indicated is much less than it is when both are exposed, or when one alone is exposed while the other is disconnected from it. (A description of such tinfoil cell will be found in the 'Philosophical Magazine' and in the 'Proceedings of the Physical Society'.)

This fact now enables us to see the importance of preventing the liquid from entering the glass tube AB, which contains the conducting wire P, for it is clear that, when the light is incident at A, the liquid which has crept into the tube round the aluminium wire will convey a portion of the negative charge imparted to the liquid in the cell to the wrong pole of the electrometer, and will thus diminish the effect of the light.

The capillary entrance of the liquid into the tube AB may, of course, be prevented by sealing into the tube a *platinum* instead of an *aluminium* wire, and coating the end of it with the selenium layer. But, unfortunately, platinum is not so good a base for the selenium as is aluminium, owing, almost certainly, to the fact that selenium enters into chemical composition with platinum, while it does not do so with aluminium, or with some other metals which, possibly, may yet be used.

The entrance of the liquid could also be prevented by using a platinum wire instead of an aluminium one, and then coating the end of the platinum wire at A with a deposit of aluminium; but, although this is doubtless possible, success in the attempt has not yet been attained.

Air-tightness is another essential condition of the constancy of these photo-electric cells, for it is found that in cells which are not quite air-tight the resistance of the cœnanthol increases very much after a few weeks, probably owing to the oxidation of the liquid by the air; and this great increase of resistance promotes sluggishness in the response of the cell to the action of light.

An examination of a seleno-aluminium cell with the various portions of the spectrum of lime-light shows that the cell is sensitive to all the rays from the end of the red, and below it, to beyond the violet, the maximum E.M.F. being produced in the yellow: but the magnitude of the E.M.F. does not vary very greatly until the violet is reached. In this respect the seleno-aluminium cell differs from all other photo-electric cells, for the sensitiveness of most of the latter is almost wholly confined to the blue. It may be mentioned, however, that a cell formed by immersing clean silver plates in a solution of eosine gives electromotive forces of opposite signs for the red and the blue rays.

The energy incident on a photo-electric cell has been found to be

proportional to the square of the electromotive force generated. If one candle held at a certain distance from the cell gives a difference,  $E$ , of potential between the poles  $P, P'$ , two candles close together will be found to give a difference  $E\sqrt{2}$ . Or, if a candle be tried at different distances from the cell, the difference of potential will be found to vary inversely as the distance.

*Intrinsic Energies of Stars.*

If  $I$  is the total amount of energy radiated into space in any time by a star at the distance  $r$  from the earth, the quantity received on any given surface on the earth will be proportional to  $I/r^2$ ; and if  $E$  is the electromotive force which this generates in a given cell, we have

$$I/r^2 = kE^2 \dots\dots\dots (1),$$

where  $k$  is some constant. Hence, if  $I'$  is the intrinsic energy of another star at the distance  $r'$ , and  $E'$  the corresponding E.M.F.,

$$I'/r'^2 = kE'^2 \dots\dots\dots (2),$$

from which we have

$$\frac{I}{I'} = \frac{E^2 r'^2}{E'^2 r^2} \dots\dots\dots (3).$$

Hence, if the parallaxes of the two stars are known, say  $p$  and  $p'$  respectively, we have

$$\frac{I}{I'} = \left( \frac{E p'}{E' p} \right)^2 \dots\dots\dots (4).$$

When it is desired to compare the energy of a star with that of the sun, we must know the area of the sensitive layer,  $A$ , Fig. 1, of selenium in the cell. Let this be  $a$ , and let  $A$  be the area of the aperture of the telescope.

Then, since it is not desirable to concentrate on the selenium the amount of solar light which falls on the large area  $A$ , we must turn the cell to the sun without the aid of the telescope. Let  $E$  be the E.M.F. observed,  $S$  the intrinsic energy of the sun, and  $r$  the distance of the sun from the earth. Then

$$a \frac{S}{r^2} = kE^2 \dots\dots\dots (5),$$

while, for any star whose distance is  $R$ , giving an E.M.F. equal to  $e$ ,

$$A \frac{I}{R^2} = ke^2 \dots\dots\dots (6).$$

$$\therefore \frac{1}{S} = \frac{a}{A} \left( \frac{eR}{Er} \right)^2 \dots\dots\dots (7).$$

As the electromotive force produced by the light of the sun falling directly on the cell is probably too large, it will be desirable to diminish its intensity by taking it through a small measured aperture and placing the cell at a known distance behind.

*The Electrometer employed.*

The instrument employed for measuring the electromotive forces generated by the light of different stars is a quadrant electrometer differing from the forms in ordinary use in having its quadrants made of aluminium, two of these being supported on brass pillars connected with the case of the electrometer and always earthed, while the other two are supported on pillars of melted quartz. The quadrant box is about 2 cm. high and 5 in diameter; the needle is of thin aluminium foil, cut into the peculiar shape figured in Clerk Maxwell's 'Electricity and Magnetism,' and is suspended by a quartz fibre about 9 cm. long. The needle and quadrants are surrounded by a thick metal case, and the instrument is both air-tight and induction-tight.

It had been intended to use with the electrometer an air condenser consisting chiefly of two gilt brass plates, each about 15 cm. in diameter, to multiply the potentials indicated by the electrometer; but at present there are difficulties in the way of its employment, and the measures made on this occasion were made by the electrometer alone.

Both instruments were constructed with the aid of the Government Grant administered by a Committee of the Royal Society and were made by Mr. Paul of Hatton Garden.

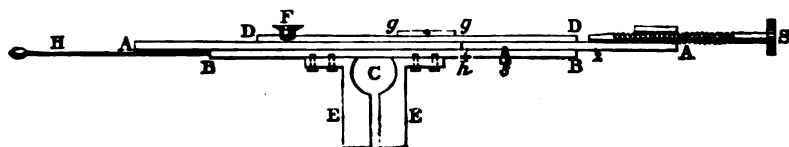
*The Telescope.*

This was Mr. Wilson's 2-feet reflector, which was at first used as a Cassegrain instrument, and subsequently as a Newtonian, the cell-carrier being in each case fixed to the telescope in place of the eyepiece.

*The Cell-Carrier.*

Fig. 3 represents the cell-carrier in plan.

FIG. 3.



A thin plate of brass AA with a circular hole of about .75 cm. diameter in the centre of its vertical face (not represented in the figure) is fitted with a screw, S, at one end. This screw pushes forward another thin brass plate, DD, which moves backwards and forwards in a grooved space in the plate AA. The plate DD has likewise a hole in its centre and is moved in one direction by S and in the opposite direction by the finger applied to a screw F, attached to DD near one end. A small thin brass plate, *gg*, is attached to DD, and can move up and down (i.e., at right angles to the plane of the figure) in a grooved space in DD by means of a screw which is not represented in the figure. This plate *gg* has also a hole in its centre over which is cemented a thin circular glass plate carrying crossed spider lines, represented in the figure by +. The point of intersection of these cross-lines is capable of being brought into any desired position by means of the horizontal motions of the plate DD and the vertical motions of *gg*.

To the face of AA opposite to that on which DD moves is attached a thin brass plate, BB, which moves in a grooved space in AA. Through BB passes a screw, *s*, which penetrates a short distance into a special groove in BB which is terminated at the points marked 1 and 2. These points are, therefore, stops to determine the extreme positions of the sliding plate BB on the fixed plate AA.

The plate BB has a circular hole of about .75 cm. in diameter in its middle, and also another, *h*, a little to the side. Over *h* is cemented a thin plate of glass on which can be marked two cross-lines marked + in the figure, or a dot.

An ebonite block with a cylindrical hole, C, into which fits the cell represented in fig. 2 is screwed to BB just over the central hole in BB.

The plate BB is moved backwards and forwards by a projecting handle, H.

The fixed plate AA is screwed to a stout cylindrical tube about two inches long (not represented), and this tube fits on the telescope instead of the usual eye-piece. AA can be adjusted, if necessary, to various positions relative to this tube, i.e., relative to the telescope.

To set the apparatus for a star, the procedure is this: move the plate BB (and with it of course the cell) by the handle H until it is stopped by the stop 2; move the intersection of the cross-lines on *gg* by means of the screw S and the screw which moves *gg* vertically until this point of intersection is exactly opposite the centre of the sensitive surface of the cell (which is, of course, visible through all the holes in the plates); bring back BB by means of H until it is stopped by the stop 1; the glass plate covering the hole *h* is now visible opposite *gg*—or rather through *h* we can see the intersection

of the cross-lines on *gg*; mark on the glass at *h* with a fine brush point a dot exactly opposite the intersection of the cross-lines of *gg*; remove *gg* out of the field of view by the vertical motion of the screw attached to it and to the plate DD—this being done in order that the light of the star may not have to pass through the glass plate on *gg*. This completes the adjustment.

To use the instrument with a star, keep BB stopped by the stop 1, so that the mark or dot at *h* is in position to receive the image of the star. Suppose, then, that by the adjustment of AA relatively to the telescope the image of the star falls exactly on this mark. Now by means of the handle H move BB until it is stopped by the stop 2. This brings the centre of the sensitive surface into the position occupied by the mark at *h*, i.e., the image of the star is now falling on the sensitive surface of the cell.

When we desire to throw off the star, we can do so by moving BB with the handle H until it is stopped by the stop 1; but it is better to effect this by moving the telescope itself in declination, without going near the cell, until the star is out of the field, as indicated by the finder; in this latter way the cell suffers no disturbing effects of temperature, &c.

#### *Connection of Cell with Electrometer.*

One pole (the insensitive) of the cell was connected with earth by a wire attached to a gas-pipe, while the other (that of the sensitive surface) was attached to a fine uncovered copper wire, carefully insulated throughout, which passed down through a shellac plug in the floor of the observation room to the electrometer in the room below.

The readings of the electrometer deflections caused by the light of the stars were made in the lower room by Professor Fitzgerald, while Mr. Wilson and I attended to matters upstairs. But in this part of the work the services of my two colleagues were of very much greater use than mine.

#### *The Observations.*

Regulus was the first star taken, on the night of the 11th April, and only two observations of the deflection on the electrometer scale produced by its light were made before proceeding to an examination of Arcturus for comparison. It is thought desirable to show in tabular form a few of the results obtained. A Leclanché cell produced on the scale a deflection of 530 mm., the scale being about 7 feet from the electrometer.

Every photo-electric cell of the type previously described and of maximum sensitiveness has a certain native or disturbing E.M.F.,

| Experiment. | Scale reading,<br>star off cell. | Scale reading,<br>star on cell. | Scale reading,<br>star off cell. | Difference<br>from mean. |
|-------------|----------------------------------|---------------------------------|----------------------------------|--------------------------|
| 1           | 150                              | 146·5                           | 151                              | 4·0                      |
| 2           | 151                              | 146·0                           | 150                              | 4·5                      |

which is always opposed to the E.M.F. generated by light. In this case it was represented by 11·2 mm.

The second column of this table contains the number on the scale at which the spot stood when the cell was in the dark, i.e., when the only E.M.F. in it was its disturbing E.M.F.; the second column contains the number to which the spot moved when the light of the star was allowed to fall on the sensitive surface in the cell.

It will thus be seen that the E.M.F. due to Regulus in these two experiments was represented by the number 4·25, or about 4·25/530 of a Leclanché.

Arcturus when tried gave the following results, after a few preliminary observations (which were tried in the case of Regulus also, and which are rendered necessary after the disturbances caused in shifting the telescope, &c.) :—

| Experiment. | Reading, star<br>off. | Reading, star<br>on. | Reading, star<br>off. | Difference<br>from mean. |
|-------------|-----------------------|----------------------|-----------------------|--------------------------|
| 1           | 135·5                 | 128·0                | 135·5                 | 7·5                      |
| 2           | 135·5                 | Clouds               | came                  | up.                      |
| 3           | 145·0                 | 138·0                | 143                   | 6·0                      |
| 4           | 143·0                 | Clouds               | came                  | up.                      |
| 5           | 136·0                 | 137·0                | Found star            | off the wires.           |
| 6           | 139·0                 | 132·5                | 139                   | 6·5                      |
| 7           | 139·0                 | 130·0                | 135                   | 7·0                      |
| 8           | 135·0                 | 127·0                | 134                   | 7·5                      |

These observations were somewhat irregular, because the sky was not quite clear during most of the observations, but was getting clearer as they proceeded.

The last was probably the best observation, and in that case Arcturus produced a voltage of about 0·02.

An experiment was made to try whether, if the cell were exposed to the sky in the neighbourhood of the star, but not to the star itself, any effect was produced, and a deflection of about 0·5 mm. in the direction *opposed* to that of the deflection caused by light was constantly observed.

If we take this effect into account, we have as the deflections due to

|                |      |
|----------------|------|
| Regulus .....  | 4.75 |
| Arcturus ..... | 8.00 |

Assuming now the latest determinations of the parallaxes of these stars to be

|                |        |
|----------------|--------|
| Regulus .....  | 0.093" |
| Arcturus ..... | 0.018" |

we have from equation (4) the ratio of their respective intrinsic energies,  $I, I'$ , the result

$$I/I' = 75.72,$$

showing that Arcturus radiates into space about  $75\frac{3}{4}$  times as much energy as Regulus in a given time.

The telescope was next turned to the star  $\gamma$  Boötis, with the result:—

| Experiment. | Reading, star off. | Reading, star on. | Reading, star off. | Difference from mean. |
|-------------|--------------------|-------------------|--------------------|-----------------------|
| 1           | 133.5              | 133               | 133.5              | 0.5                   |
| 2           | 133.5              | 133               | 133.5              | 0.5                   |

If the radiation to the sky is added, the star's deflection amounts to 1 mm., i.e., a voltage of 0.0028.

This observation is, however, recorded merely for the purpose of showing that a comparatively faint star is able to give an unmistakable E.M.F.

The telescope was next turned on Saturn, whose image was so large that the ends of his rings were probably off the sensitive surface in the cell. The observation was:—

| Experiment. | Reading, star off. | Reading, star on. | Reading, star off. | Difference from mean. |
|-------------|--------------------|-------------------|--------------------|-----------------------|
| 1           | 129                | 125.5             | 129                | 3.5                   |
| 2           | 129                | 125.0             | 129                | 4.0                   |
| 3           | 129                | 122.5             | 126                | 3.75                  |

Between the second and third of these experiments the cell had been slightly disturbed, so that the last is not very satisfactory, although it gives the mean result.

The glare from the moon was now very distinctly apparent, and its effect seemed to be a deflection of about 0.5 mm., which must be deducted from Saturn's effect. This latter would, then, be represented by about 3.25 mm., or a voltage of 0.009.

The next observations were made on the night of Friday, the 12th, and on this night the atmosphere was very hazy, although many stars were visible. It is not necessary to enter into the details of observations on such a night; nevertheless, it may be interesting to see the effects which were observed.

For the following bodies the mean deflections were as tabulated:—

|                      |          |
|----------------------|----------|
| Jupiter .....        | 6.33 mm. |
| $\alpha$ Cygni ..... | 0.50 „   |
| Vega .....           | 2.26 „   |
| Arcturus.....        | 2.50 „   |
| Regulus .....        | 1.10 „   |

A standard candle placed at a distance of 9 feet from the cell produced a deflection of 11 mm.

Nothing of a quantitative nature can be deduced from these results owing to the presence (and unequal distribution) of haze, the effect of which was to make Vega produce a deflection less than that of Arcturus.

On the night of the 14th the sky was clearer, and while Arcturus gave a very fairly constant deflection of 4.5 mm., Regulus gave 1.85 mm.,  $\epsilon$  Boötis 0.32 mm.,  $\alpha$  Coronæ 0.6 mm., and  $\beta$  Herculis something less than 0.2 mm.

The night of the 15th was, for a short time, much clearer, and during this time the following more reliable measures were made:—

|                    |                                  |
|--------------------|----------------------------------|
| Arcturus.....      | 8.2 mm. (mean of 4 observations) |
| Saturn.....        | 5.6 „ ( „ 4 „ )                  |
| Vega .....         | 11.5 „ ( „ 2 „ )                 |
| Candle at 9 feet.. | 10.0 „                           |
| Leclanché cell ... | 513.0 „                          |

It would be interesting to compare the intrinsic energies of Arcturus and Vega, but the parallax of Vega seems to be almost more uncertain than that of Arcturus. It is given in Young's 'Astronomy' as 0.16, while Miss Clerke gives Elkin's value as "0.034?" If the first of these is taken, with the value 0.018 for Arcturus, equation (4) gives the intrinsic energy of Arcturus equal to 38.2 times that of Vega; but if the second is taken, this number becomes only 1.8.

*Comparison with the Photometric Method.*

In the ordinary method of comparison of "magnitudes," if B and B' are the brightnesses of two stars whose magnitudes are  $m$  and  $m'$ , respectively, we have, by definition,

$$\log \frac{B}{B'} = \frac{1}{2.5} (m' - m) \dots\dots\dots (5).$$

Now, taking Arcturus and Regulus as of the magnitudes 0.3 and  $m'$ , respectively, and the electromotive forces of their lights as 8 and 4.75 (determination of April 11, previously cited), we have

$$\log \left( \frac{8}{4.75} \right)^2 = \frac{1}{2.5} (m' - 0.3),$$

$$\therefore m' = 1.43.$$

The magnitude of Regulus, as a matter of fact, is variously cited as from 1.42 to 1.7; thus the amount of correspondence between the photo-electric and photometric methods is seen.

*Concluding Remarks.*

Among the few bright stars which we found available was Procyon, and even this star offered an opportunity for observation during a very limited time, owing to mechanical hindrances in the Observatory. The stars in the Great Bear shone brilliantly, and, under favourable circumstances, their light could have been easily measured. The constellation was, however, so nearly vertical, that the aperture in the roof of the Observatory was not sufficiently wide to suit the aperture of the telescope, and hence no observation of any of these stars was attempted.

On one night observations of Procyon and Regulus were taken. The readings were much smaller than had been anticipated from the great sensitiveness of the cell and electrometer. When these observations were completed, the cell was exposed to a candle at 9 feet, and the effect was so small, that it was evident that some accidental circumstance was intervening. The cell was, therefore, taken down from the telescope and examined, with the result that we found an opaque movable portion of the cell holder covering a portion of the sensitive surface in the cell. This was at once removed, and then the candle, Arcturus, and Vega gave the large deflections quoted in the observations of the 15th. It was, however, then too late to get Procyon again. But the observations which had been made with this star and Regulus, while the partial obstruction of the cell remained, gave the mean of their deflections as

|               |          |
|---------------|----------|
| Regulus ..... | 1.27 mm. |
| Procyon ..... | 1.90 ..  |

Now, although the accidental obstruction renders this comparison unsatisfactory, it is remarkable that these numbers accord fairly well with the "magnitudes" of the two stars, as given by Miss Clerke ('System of the Stars,' Appendix, Table III). Thus, the "magnitude" of Regulus is given as 1.4, and that of Procyon as 0.5.

Now, in equation (5), if we put  $B/B' = (190/127)^2$ , and assume the magnitude of Regulus = 1.4, while that of Procyon =  $m$ , we have

$$5 \log \frac{190}{127} = 1.4 - m,$$

$$\therefore m = 0.53,$$

which is a rather close coincidence.

On the same data equation (4) gives the result

Intrinsic energy of Regulus =  $3.6 \times$  intrinsic energy of Procyon.

It is hoped that these measurements will be resumed about the end of next September, at Daramona, by the same observers; and, in the meantime, some improvements will be effected in the cell-holder which will facilitate observation. An endeavour will also be made to improve the cell itself in the directions indicated at the beginning of this Report.

The experiments prove conclusively that there is little difficulty in obtaining fairly accurate measurements of the light of stars of the first and second "magnitudes," even without the employment of a multiplying condenser or a larger telescope. A telescope with an aperture of 5 or 6 feet would certainly annex a very great number of stars to the list.

It is right to put on record the fact that the first photo-electric observations of planets and stars were made by Mr. Monck, in Dublin, in the year 1892, in conjunction with Professor Fitzgerald. My cells were at that time much less sensitive than the present ones; and, for reasons set forth in this paper, their sensitiveness fell off after about six hours. The liquid in those cells was acetone, and the aluminium on which the selenium was deposited was not insulated from the liquid. Nevertheless, Mr. Monck and Professor Fitzgerald were able to observe electromotive forces due to the light of Venus, Jupiter, and, I think, Mars. Mr. Monck's telescope is a refractor of 9 ins. aperture, so that large results were not to be expected. These observers were not quite certain whether Vega and Capella produced measurable effects or not; but their observations were much interfered with by draughts of air, and other things, in their observatory.

III. "The Complete System of the Periods of a Hollow Vortex Ring." By H. C. POCKLINGTON, B.A., Fellow of St. John's College, Cambridge. Communicated by Professor LARMOR, F.R.S. Received April 23, 1895.

(Abstract.)

The author discusses the stability of a hollow annular vortex in an infinite perfect liquid, and also the effect of an electric charge on the steady motion and the stability of such a vortex. It is known that a hollow vortex ring (without electric charge) is stable for such small deformations as are symmetrical about the axis of symmetry of the ring, and for such as consist in displacement of the axis of the hollow without alteration of the size or shape of its cross section. This investigation shows that, in addition to the fluted and sinuous vibrations above referred to, the vortex is capable of beaded vibrations, in which the hollow is enlarged and contracted at regular intervals along its length, and also of vibration of a more general type, in which the displacement at any moment consists of waves on the surface of the hollow, of which the crests are circles parallel to the axis of the hollow, and the amplitude a sine or cosine of a multiple of the azimuth angle. The periods of these vibrations are found and proved to be real. Since, as is easily seen, any displacement of the surface can be compounded by displacement of the various types here mentioned, the vortex is stable for all displacements of its surface.

When an attempt is made to explain matter as composed of atoms which consist of such vortex rings, a difficulty is found at the outset if the theory is applied to gases. On the kinetic theory of gases, a theory which, over a wide range, gives results in accordance with those of experiment, the energy of an atom varies as the square of its velocity. The energy of a vortex ring, however, decreases as its velocity increases. Thus the single vortex atom theory is likely to yield results that disagree with experimental results when applied to gases. If, however, the electric charge is taken into account, this defect can be, to a certain extent, remedied.

The case where the electricity resides on the surface, supposed conducting, of the hollow of the vortex ring is worked out on the hypothesis that the period of an electrical oscillation is so small that the electricity has at any time its equilibrium distribution. It is found that, in the case of such a ring, however small the charge may be, the velocity of the ring can be decreased, made to vanish, and finally to change sign by decreasing the radius of the ring. At the same time, the energy diminishes, attains a minimum when the velocity of the vortex is zero, and then increases. If therefore the

vortex atom always has nearly the size that corresponds to minimum energy, its energy, neglecting a constant term, varies as the square of its velocity. This relation, however, only holds through a small range. On investigation of the periods of the electrified vortex it is found that it is always unstable for some types of disturbance.

The method employed throughout the investigation is to refer the circumstances of the motion to toroidal co-ordinates. The steady motion of the fluid is then expressed by means of a Stokes' current function, and the disturbances of the steady motion, in the case of vibration, by means of a potential function. The periods of the different oscillations, when real, are given as  $2\pi/p$ , where  $p$  is given by

$$p^2 + 2pn \frac{\mu}{2\pi e^2} + n(n-1) \left\{ 1 + \frac{E^2}{\pi \rho a^2 \mu^2} \right\} \left( \frac{\mu}{2\pi e^2} \right)^2 = 0$$

in the case of the general oscillation, where there are  $n$  waves in the circumference of the hollow;

$$p^2 = \left( \frac{\mu}{2\pi e^2} \right)^2 \left\{ \frac{1}{L' - \frac{1}{2}\gamma_m} + \frac{(L' - \frac{1}{2}\gamma_m)^2 + 1}{L' - \frac{1}{2}\gamma_m} \frac{E^2}{\pi \rho a^2 \mu^2} \right\}$$

in the case of beaded vibrations, where  $m$  is the number of enlargements in the circumference of the ring,  $L' = \log sa/e$  and

$$\gamma_m = 4\{1 + 1/3 + 1/5 + \dots - 1/(2m-1)\};$$

$$p^2 = \left( \frac{\mu}{2\pi e^2} \right)^2 \frac{1}{L' - \frac{1}{2}\gamma_m} \left\{ 1 + \frac{E^2}{\pi \rho a^2 \mu^2} \right\}$$

in the case of the pulsations, where  $\mu$  is the circulation of the ring,  $a$  its radius,  $e$  the radius of the cross-section of the hollow,  $\rho$  the density of the liquid,  $E$  the electric charge on the ring. In the case of the general oscillation, in which the disturbance consists of  $n$  waves parallel to the axis of the ring, the amplitudes of which vary as cosine or sine of  $m$  times the azimuth angle, the value of  $p$  must be modified if  $m$  is large. The formula for  $p$  then is

$$p^2 + 2pn \frac{\mu}{2\pi e^2} + \left\{ n^2 + \frac{2mbK'_n(2mb)}{K_n(2mb)} + \frac{[2mbK'_n(2mb)]^2 + [K_n(2mb)]^2}{2mbK'_n(2mb) \cdot K_n(2mb)} \frac{E^2}{\pi \rho a^2 \mu^2} \right\} \left( \frac{\mu}{2\pi e^2} \right)^2 = 0.$$

IV. "India's Contribution to Geodesy." By General J. T. WALKER, R.E., C.B., F.R.S., LL.D., late Surveyor-General of India. Received April 27, 1895.

(Abstract.)

This paper gives a summary of the operations which have been completed up to date in India to furnish the necessary data of the lengths and the amplitudes of meridional and longitudinal arcs for a mathematical determination of the mean figure of the earth. It shows that geodetic investigation has already greatly influenced the operations of the Great Trigonometrical Survey of India, from its very commencement at the beginning of the present century. It describes the principal triangulation which has been laid out over the whole face of the country, mostly in meridional and longitudinal chains, and shows how it has been finally reduced and made consistent throughout by processes of calculation which are fully set forth in the published accounts of the operations. Brief statements of the discrepancies which were met with at the base-lines and the sides of junction of the chains of triangles, and the average values of the angular corrections which had to be applied to satisfy the discrepancies and produce harmony and consistency throughout, are given to indicate the extent to which the triangulation may be relied on for geodetic investigations. Then the astronomical determinations of latitude, and those of differences of longitude by the electro-telegraphic method, are described and the results are compared with the corresponding geodetic determinations from the triangulation.

All the facts of observation having been given, the question is considered of the extent to which the astronomical determinations have been influenced by local deflections of the plumb line. The views which have been put forward as regards the attraction of the great Himalayan Mountains on the plumb line are considered; it is shown that the magnitudes of the attractions computed on the theory of gravitation are considerably greater than is necessary to explain the discrepancies between the astronomical and the geodetic results, and this fact is fully explained by the Indian pendulum operations, which have thrown great light on the constitution of the earth's crust. They indicate that there is an excess of density under the sea-level and a deficiency above that level which increases to a very notable magnitude at a high altitude in the Himalayas; thus, therefore, there must be a condensation of matter under ocean beds, and an attenuation under mountains; and consequently points must be sooner or later reached at which the positive attraction of a mountain mass is cancelled by the negative attraction of the mass under

the mountain. The geodetic operations of the survey show the same thing in the Southern Peninsula, which is the part of India closest to the ocean. Here the latitude stations almost invariably show deflection of the plumb-line towards the ocean, instead of away from it, as might naturally be expected. Here, too, the astronomical amplitudes of the longitudinal arcs are all too small, with one exception, which can be readily explained, thus also showing deflection of the plumb-line towards the ocean at the stations on the coast-line.

It is not difficult to draw a line beyond which Himalayan attraction need not be feared. Excluding all stations obviously under Himalayan influence, 148 latitudes and 50 longitudinal arcs remain for employment—with similar data obtained in other parts of the world—in determining the figure of the earth. But on comparing the astronomical with the geodetic determinations it is seen that the former are often grossly influenced by deflections of the plumb-line at points where there is nothing visible to suggest disturbance. Thus, at the Colaba Observatory, Bombay, there is probably 8" deflection to the north, while at the Madras Observatory there is probably as great deflection to the south, neither place having any apparent source of meridional attraction; here the astronomical amplitude is 16" less than the geodetic, a difference which is fully twenty times greater than any error that can have been made in the triangulation. There are also discrepancies of corresponding magnitude in the longitudinal arcs at places where there is no apparent source of attraction on the prime vertical.

Clearly, therefore, no single astronomical determination can be regarded as sufficiently free from deflection of the plumb-line to be safely employed in an investigation of the figure of the earth. But all investigations hitherto made have rested on single determinations. This, however, must be almost certainly due to the paucity of data; had a sufficiency of astronomical results been forthcoming to indicate the presence of large local disturbances of the plumb-line and emphasise the necessity to eliminate these disturbances as far as possible before proceeding to employ the astronomical results, there can be no doubt that some attempts at elimination would have been made.

There is only one way of eliminating these errors, and that is to combine the astronomical observations together in groups, each containing determinations at as many places in a given belt of latitude as possible, and to take the mean of the group; for the mean astronomical latitude of a number of points may certainly be assumed to be far more free from deflection than that of any single point. The geodetic latitudes of the same points, as obtained from the triangulation, would be similarly combined; and thus the most accurate

data for a mathematical investigation of the earth's figure would be obtained.

In this way eight meridional arcs, extending from latitude  $8^{\circ} 43'$  to latitude  $30^{\circ} 9'$ , have been produced for India; they rest on an average of over sixteen stations for each of the fundamental latitudes, and not, as usual, on one more station than the number of arcs. The amplitudes of these arcs vary from  $2^{\circ} 23'$  to  $3^{\circ} 38'$ , ranging in magnitude with the magnitudes of the mean ultimate local attractions, in accordance with a rule which has been derived from the formula which expresses the error of a geodetic arc in terms of the errors of the major and minor axes. Two great longitudinal arcs, exceeding  $24^{\circ}$  in amplitude, have been produced by summations of the observed smaller arcs, thus eliminating all errors caused by deflections of the plumb-line at the intermediate stations, and retaining only those at the eastern and western extremities of the entire arcs.

These eight meridional and two longitudinal arcs are put forward as the most valuable data for the investigation of the figure of the earth which have yet been published. They are the result of operations which have been made in India during a period of over ninety years, always with the cordial support and approval of the Government of India.

The Society adjourned over Ascension Day to Thursday, May 30.

*Presents, May 16, 1895.*

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May 30, 1895.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of notice sent to the Fellows, an election was held to fill the vacancy upon the Council occasioned by the retirement of Professor A. H. Green.

The Statutes relating to the election of the Council and the Statute relating to the election of a Member of Council upon the occurrence of a vacancy were read, and Dr. Armstrong and Mr. R. H. Scott having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken and the Rev. Thomas George Bonney was declared duly elected.

The following Papers were read:—

- I. "On the Temperature Variation of the Thermal Conductivity of Rocks." By LORD KELVIN, P.R.S., and J. R. ERSKINE MURRAY, B.Sc., 1851 Exhibition Scholar. Received May 24, 1895.

§ 1. The experiments described in this communication were undertaken for the purpose of finding temperature variation of thermal conductivity of some of the more important rocks of the earth's crust.

§ 2. The method which we adopted was to measure, by aid of thermoelectric junctions, the temperatures at different points of a flux line in a solid, kept unequally heated by sources (positive and negative) applied to its surface, and maintained uniform for a sufficiently long time to cause the temperature to be as nearly constant at every point as we could arrange for. The shape of the solid and the thermal sources were arranged to cause the flux lines to be, as nearly as possible, parallel straight lines; so that, according to Fourier's elementary theory and definition of thermal conductivity, we should have

$$\frac{k(M, B)}{k(T, M)} = \frac{[v(M) - v(T)] \div MT}{[v(B) - v(M)] \div BM},$$

where T, M, B denote three points in a stream line (respectively next to the top, at the middle, and next to the bottom in the slabs and columns which we used);  $v(T)$ ,  $v(M)$ ,  $v(B)$  denote the steady tem-

peratures at these points; and  $k(T, M)$ ,  $k(M, B)$ , the mean conductivities between T and M, and between M and B respectively.

§ 3. The rock experimented on in each case consisted of two equal and similar rectangular pieces, pressed with similar faces together. In one of these faces three straight parallel grooves are cut, just deep enough to allow the thermoelectric wires and junctions to be embedded in them, and no wider than to admit the wires and junctions (see diagram, § 8 below). Thus, when the two pieces of rock are pressed together, and when heat is so applied that the flux lines are parallel to the faces of the two parts, we had the same result, so far as thermal conduction is concerned, as if we had taken a single slab of the same size as the two together, with long fine perforations to receive the electric junctions. The compound slab was placed with the perforations horizontal, and their plane vertical. Its lower side, when thus placed, was immersed under a bath of tin, kept melted by a lamp below it. Its upper side was flooded over with mercury in our later experiments (§§ 6, 7, 8), as in Hopkins' experiments on the thermal conductivity of rock. Heat was carried off from the mercury by a measured quantity of cold water poured upon it once a minute, allowed to remain till the end of a minute, and then drawn off and immediately replaced by another equal quantity of cold water. The chief difficulty in respect to steadiness of temperature was the keeping of the gas lamp below the bath of melted tin uniform. If more experiments are to be made on the same plan, whether for rocks or metals, or other solids, it will, no doubt, be advisable to use an automatically regulated gas flame, keeping the temperature of the hot bath in which the lower face of the slab or column is immersed at as nearly constant a temperature as possible, and to arrange for a perfectly steady flow of cold water to carry away heat from the upper surface of the mercury resting on the upper side of the slab or column. It will also be advisable to avoid the complication of having the slab or column in two parts, when the material and the dimensions of the solid allow fine perforations to be bored through it, instead of the grooves which we found more readily made with the appliances available to us.

§ 4. Our first experiments were made on a slate slab, 25 cm. square and 5 cm. thick, in two halves, pressed together, each 25 cm. by 12.5, and 5 cm. thick. One of these parts cracked with a loud noise in an early experiment, with the lower face of the composite square resting on an iron plate heated by a powerful gas burner, and the upper face kept cool by ice in a metal vessel resting upon it. The experiment indicated, very decidedly, less conductivity in the hotter part below the middle than in the cooler part above the middle of the composite square slab. We supposed this might possibly be due to the crack, which we found to be horizontal and below the middle,

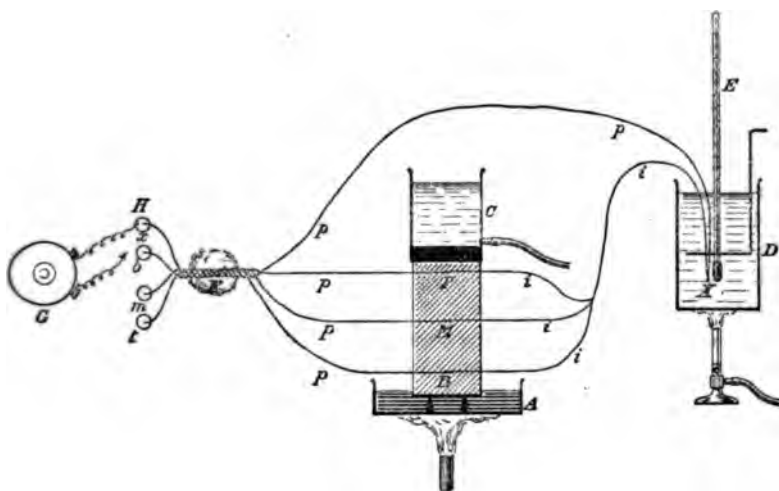
and to be complete across the whole area of  $12\frac{1}{2}$  cm. by 5, across which the heat was conducted in that part of the composite slab; and to give rise to palpably imperfect fitting together of the solid above and below it. We therefore repeated the experiment with the composite slab turned upside down, so as to bring the crack in one half of it now to be above the middle, instead of below the middle, as at first. We still found for the composite slab less conductivity in the hot part below the middle than in the cool part above the middle. We inferred that, in respect to thermal conduction through slate across the natural cleavage planes, the thermal conductivity diminishes with increase of temperature.

§ 5. We next tried a composite square slab of sandstone of the same dimensions as the slate, and we found for it also decisive proof of diminution of thermal conductivity with increase of temperature. We were not troubled by any cracking of the sandstone, with its upper side kept cool by an ice-cold metal plate resting on it, and its lower side heated to probably as much as  $300^{\circ}$  or  $400^{\circ}$  C.

§ 6. After that we made a composite piece, of two small slate columns, each 3.5 cm. square and 6.2 cm. high, with natural cleavage planes vertical, pressed together with thermoelectric junctions as before; but with appliances (§ 10 below) for preventing loss or gain of heat across the vertical sides, which the smaller horizontal dimensions (7 cm., 3.5 cm.) might require, but which were manifestly unnecessary with the larger horizontal dimensions (25 cm., 25 cm.) of the slabs of slate and sandstone used in our former experiments. The thermal flux lines in the former experiments on slate were perpendicular to the natural cleavage planes, but now, with the thermal flux lines parallel to the cleavage planes, we still find the same result, smaller thermal conductivity at the higher temperatures. Numerical results will be stated in § 12 below.

§ 7. Our last experiments were made on a composite piece of Aberdeen granite, made up of two columns, each 6 cm. high and 7.6 cm. square, pressed together, with appliances similar to those described in § 6; and, as in all our previous experiments on slate and sandstone, we found less thermal conductivity at higher temperatures. The numerical results will be given in § 12 below.

§ 8. The accompanying diagram represents the thermal appliances and thermoelectric arrangement of §§ 6, 7. The columns of slate or granite were placed on supports in a bath of melted tin with about 0.2 cm. of their lower ends immersed. The top of each column was kept cool by mercury, and water changed once a minute, as described in § 3 above, contained in a tank having the top of the stone column for its bottom and completed by four vertical metal walls fitted into grooves in the stone and made tight against wet mercury by marine glue.



Iron wires are marked *i*.

Platinoid wires are marked *p*.

B, M, T. Thermoelectric junctions in slab.

X. " " oil bath.

A. Bath of molten tin.

C. Tank of cold water.

D. Oil bath.

E. Thermometer.

F. Junctions of platinoid and copper wires. The wires are insulated from one another, and wrapt all together in cotton wool at this part, to secure equality of temperature between these four junctions, in order that the current through the galvanometer shall depend solely on differences of temperature between whatever two of the four junctions, X, T, M, B, is put in circuit with the galvanometer.

G. Galvanometer.

H. Four mercury cups, for convenience in connecting the galvanometer to any pair of thermoelectric junctions.

*x, b, m, t*, are connected, through copper and platinoid, with X, B, M, T, respectively.

§ 9. The temperatures,  $v(B)$ ,  $v(M)$ ,  $v(T)$  of B, M, T, the hot, intermediate, and cool points in the stone, were determined by equalising to them successively the temperature of the mercury thermometer placed in the oil-tank, by aid of thermoelectric circuits and a galvanometer used to test equality of temperature by nullity of current through its coil when placed in the proper circuit, all as shown in the diagram. The steadiness of temperature in the stone was tested by keeping the temperature of the thermometer constant, and observing the galvanometer reading for current when the junction in the oil-tank and one or other of the three junctions in the stone were placed in circuit. We also helped ourselves to attaining constancy of

temperature in the stone by observing the current through the galvanometer, due to differences of temperature between any two of the three junctions B, M, T placed in circuit with it.

§ 10. We made many experiments to test what appliances might be necessary to secure against gain or loss of heat by the stone across its vertical faces, and found that *kieselguhr*, loosely packed round the columns and contained by a metal case surrounding them at a distance of 2 cm. or 3 cm., prevented any appreciable disturbance due to this cause. This allowed us to feel sure that the thermal flux lines through the stone were very approximately parallel straight lines on all sides of the central line BMT.

§ 11. The thermometer which we used was one of Cassella's (No. 64,168) with Kew certificate (No. 48,471) for temperature from 0° to 100°, and for equality in volume of the divisions above 100°. We standardised it by comparison with the constant volume air thermometer\* of Dr. Bottomley with the following result. This is satisfactory as showing that when the zero error is corrected the greatest error of the mercury thermometer, which is at 211° C., is only 0·3°.

| Reading.            |                         | Correction to be subtracted<br>from reading of mercury<br>thermometer. |
|---------------------|-------------------------|------------------------------------------------------------------------|
| Air<br>thermometer. | Mercury<br>thermometer. |                                                                        |
| 0                   | 1·9                     | 1·9                                                                    |
| 120·2               | 122·2                   | 2·0                                                                    |
| 166·8               | 168·6                   | 1·8                                                                    |
| 211·1               | 212·7                   | 1·6                                                                    |
| 265·7               | 267·5                   | 1·8                                                                    |

§ 12. Each experiment on the slate and granite columns lasted about two hours from the first application of heat and cold; and we generally found that after the first hour we could keep the temperatures of the three junctions very nearly constant. Choosing a time of best constancy in our experiments on each of the two substances slate and granite, we found the following results:—

Slate: flux lines parallel to cleavage.

$$v(T) = 50^{\circ}\cdot 2 \text{ C.}$$

$$v(M) = 123^{\circ}\cdot 3.$$

$$v(B) = 202^{\circ}\cdot 3.$$

\* 'Phil. Mag.,' August, 1888, and 'Edinb. Roy. Soc. Proc.,' January 6, 1888.

The distances between the junctions were  $BM = 2.57$  cm. and  $MT = 2.6$  cm. Hence by the formula of § 2,

$$\frac{k(M, B)}{k(T, M)} = \frac{73.1 \div 2.6}{79.0 \div 2.57} = \frac{28.1}{30.7} = 0.91.$$

Aberdeen granite:

$$v(T) = 81^{\circ}.1.$$

$$v(M) = 145^{\circ}.6.$$

$$v(B) = 214^{\circ}.6.$$

The distances between the junctions were  $BM = 1.9$  cm. and  $MT = 2.0$  cm.

$$\frac{k(MB)}{k(TM)} = \frac{64.5 \div 2.0}{69.0 \div 1.9} = \frac{32.2}{36.3} = 0.88.$$

§ 13. Thus we see, that for slate, with lines of flux parallel to cleavage planes, the mean conductivity in the range from  $123^{\circ}$  C. to  $202^{\circ}$  C. is 91 per cent. of the mean conductivity in the range from  $50^{\circ}$  C. to  $123^{\circ}$  C., and for granite, the mean conductivity in the range from  $145^{\circ}$  C. to  $214^{\circ}$  C. is 88 per cent. of the mean conductivity in the range from  $81^{\circ}$  C. to  $145^{\circ}$  C. The general plan of apparatus, described above, which we have used only for comparing the conductivities at different temperatures, will, we believe, be found readily applicable to the determination of conductivities in absolute measure.

II. "The Kinematics of Machines." By T. A. HEARSON, M.Inst.C.E., Professor of Mechanism and Hydraulic Engineering, Royal Indian Engineering College, Coopers Hill. Communicated by Professor COTTERILL, F.R.S. Received March 19, 1895.

(Abstract.)

In this paper the author regards a machine as an embodiment of a movement. The method of construction and the proportions of the parts are not taken into consideration, except so far as may be necessary to explain the conditions requisite for the kinds of motions with which they are supposed to be endowed. All other considerations relating to form and proportion are omitted, as belonging to the subject of machine design. Neither does the author take account of the forces which actuate and oppose the movement of the machine, such matters belonging to the subject Dynamics of Machines.

The object of the paper is to analyse the movements only, and to

show the likeness and the differences between machines in similarities in the movements or the contrary.

It is claimed by the author that in those movements the principal feature of a machine resides, distinguishing it from other engineering constructions.

It is shown that all movements, however complex, are derived from the association together of some of a comparatively limited number of kinds of more or less simple motions, which take place between consecutive directly connected pieces.

Certain geometrical laws are enunciated, from which are derived the conditions necessary for the association of those motions together in one machine. It is shown that those laws preclude the existence of certain combinations of motions, and it is suggested that one may be enabled by this analysis to enumerate an exhaustive list of the possible combinations which must include all existing machines, and suggest the design of others not in existence. Moreover, by attaching to each kind of motion a suggestive symbol, a method of expressing the constitution of a machine movement by a simple formula is proposed, whereby similarities and differences between machines may be exhibited at a glance.

The author commences by considering a very simple mechanism, consisting of four bars united in one continuous linkage by four pins which have parallel axes. By imagining the length of the links to undergo variation from zero to infinity, it is shown that this simple mechanism is representative of all the simple plane mechanisms, and, by imagining other variations to occur, this same mechanism is shown to be representative of still further classes of mechanisms, in which the parts do not move in or parallel to one plane. In this simple mechanism the relative motions of consecutive pieces are either turning, when one piece revolves completely around relatively to the other, the representative symbol being the letter O, or swinging when one piece turns through a limited angle relatively to the adjoining one, represented by the letter U.

The first law enunciated, which governs the association of the O and U motions, is founded on the geometrical fact that the sum of the three angles of a plane triangle is constant, and the sum of the four angles of the quadrilateral therefore also constant. After a complete revolution the angle between the bars is considered to have been increased or diminished by  $2\pi$ . With this extension of the proposition the constancy of the sum of the angles is unimpaired.

From this it is seen to be impossible for only one motion to be turning and the other three swinging, otherwise the sum of the four angles would increase or decrease by  $2\pi$  each revolution.

The second law, which governs the association of the motions, has to do with the proportions between the length of the links necessary

to permit of complete turning. This is founded on the fact that one side of a triangle cannot be greater than the sum of the other two. From these two laws together it is shown that it is impossible to have two O's alternating with two U's.

Next it is pointed out how the U motion may be provided for by constructing a circular slotway in one piece, and shaping the other piece to fit the slotway, so that by imagining the radius of curvature of the slotway to be indefinitely increased a relative movement of reciprocating sliding motion, represented by the symbolical letter I, will be substituted for the swinging motion U. A slide being conceived to be a swing through a zero angle about an infinitely distant centre, the previously mentioned laws will apply to associations containing I motions, and it will follow that a combination of three slides and one swing is precluded by the first law.

If four slides are associated, in which all four of the links of the original mechanism are to be conceived to be infinitely long, an indeterminate motion will result comparable to the motion which would be possible if five bars were joined by pairs in a closed circuit.

One of the slides may be suppressed, and a definite motion will result from three slides.

If the foregoing analysis be compared with that due to Reuleaux, to which it bears a close resemblance, it will be seen that Reuleaux conceives that the elementary essential components of machines are the *pairs* of consecutive links which are in mutual contact, whereas it is here proposed that the relative *motions* of consecutive links should be regarded as the essential elements or components of a machine movement. Whilst the pairs of surfaces of contact of consecutive pieces should be formed to suit the kind of relative motion which those pieces are required to undergo, yet the forms of those surfaces do not themselves entirely govern the character of the motion.

Reuleaux assumes that what he calls a turning motion and the I motion are entirely governed by the forms of the surfaces of mutual contact, but shows that to ensure a more complex motion a restraint is required to be imposed by means external to the two links. Those additional means of constraint have to be included with that due to the forms of the surfaces of mutual contact in the conception of a complete pair, and often the whole mechanism is required to complete one pair contained in it.

Reuleaux does not attempt to discriminate between a turning and a swinging pair; the same pair of surfaces of mutual contact is suitable for both; the difference consists of a difference only in the rest of the mechanism, yet the difference in the two motions is most apparent, and is very important, both kinematically and also from the practical engineer's point of view.

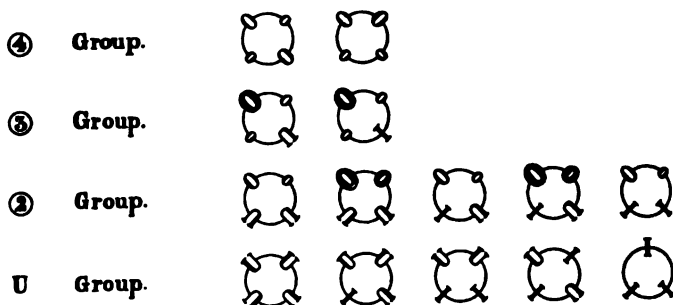
No advantage is derived from analysing a machine into parts such as pairs if it requires the whole machine to complete one of the parts.

The enunciation and the explanation of the influence of the first law previously mentioned, of the constancy of the sum of the four angles of a quadrilateral in governing the association of the OU and I motions in one mechanism, is one of the important original features of this paper.

The influence of the second law, viz., that the two sides of a triangle are together not less than the third, in limiting the association of the O, U, I motions, is now also for the first time pointed out, though Reuleaux and others have, without formally enunciating the law, made use of the fact to determine the proportions necessary for certain suggested movements.

By the application of these governing laws one is able to write an exhaustive list of all the possible combinations in one simple mechanism of the three simple O, U, I motions, and to explain why other combinations are precluded.



Fourteen distinct combinations are possible, and only fourteen. They are exhibited by the following formulæ, in which a large O associated with a small o signifies that in one case adjacent links turn relatively to one another so as to continuously increase the angle between them, and in the other to continuously diminish the angle. The double @ signifies that two complete revolutions accompany one complete to and fro swing or slide.



Following Reuleaux, the author applies the principle of the "inversion of the kinematic chain," considering it to be a continuous sequence of links in a closed circuit containing a sequence of elementary motions. In explaining what is meant by inversion, it is pointed out that relatively to an observer or user of a machine one piece is fixed. This is called the frame of the machine. Each one of the four links may in turn be made the fixed or frame link, and

although the relative motion of the four links will in all cases remain unaltered, the absolute movement, or movement relatively to the user of the machine, will in general be different for each fixing, and constitute a new machine movement. Changing the fixed or frame link is called the "inversion of the chain."

The author makes use of the term "primary pieces," originally suggested by Rankine for those links which are in sequence with and directly connected to the frame link, and shows that if, after inversion, the new primary pieces have the same kind of motions as the previous primary pieces had, the consequent machine movement is not a new one, but a repetition of a previous one.

From the mechanisms  and  only can four different machine movements be obtained by inversion. From the others 3, 2, or only 1 can be derived.

They are distinguished from one another in the formula by using a thick line for the frame link. Thus



signifies a machine movement like that employed in the crank and connecting-rod engine.



is exemplified in the oscillating engine much used in paddle-wheel steamers.



is found in Stannah's pendulum pump, and



is the movement adopted by Rigg in the design of his high speed engine. The intimacy of the relation of this engine to the preceding ones is here for the first time indicated.

In all, thirty-two and only thirty-two distinct machine movements can be derived from the fourteen previously enumerated mechanisms by inversion.

It is shown that Reuleaux's principle of inversion can be applied with more advantage and consistency if a machine movement is analysed into its component motions than if a machine is analysed into its component pairs, and the notation lends itself to a very clear exhibition of the effect of inversion.

The author next discusses the relation of cams and spur-wheel mechanisms to the foregoing kinematic chains, showing that they are the result of the suppression of one of the previous four links and the amalgamation of the two adjoining simple motions into one more complex. A comparison is also made with belt gearing and expressive formulæ suggested.

The author then passes to the consideration of machines the parts of which do not move parallel to one plane.

Reuleaux was the first to show that if the links of the previously mentioned kinematic chains be bent to the form of great circles of a sphere the axes of the connecting pins will be radial, and the previously mentioned machine movements will be possible under the modified circumstances.

In spherical motion the counterpart of what is a slide in plane motion could be obtained by a swinging motion about a pole of which the bent link is the equator. The motion is to be conceived as due to the use of two bent links, the length of one of which is a quadrant of a great circle of the sphere.

In these so-called spherical mechanisms, Law I has to be modified as follows :—

The sum of the four angles of the spherical quadrilateral varies, having a value of  $3\pi$  for a maximum and  $2\pi$  for a minimum.

This and Law II, which is the same as before, will preclude the same combinations in spherical mechanisms which were precluded in plane mechanisms.

Law I explains at once why in Hooke's joint, which is the spherical counterpart of Oldham's coupling, the angular velocity-ratio of the connected shafts is not constant, whereas in Oldham's coupling it is.

The author points out that the kinematic chain containing three slides cannot be adapted to give a movement on a sphere. The virtual construction would consist of a spherical triangle between the links of which no relative motion is possible, and there is not room on the sphere for a movement at each joint of a bent quadrilateral, the length of each side of which is equal to a quadrant. But a three-slide mechanism can be adapted to give motion on the surface of a cylinder, and it is the only one of the fourteen kinematic chains which can be so adapted, and examples of it are found in the various helical motions so largely used. (The letter V is used to represent helical motions.) This method of showing the relation between screw motions and plane motions is a novel feature of the paper.

The remaining mechanisms consist of those in which the axes of the turning and swinging motions neither meet nor are parallel. They include the motion which occurs at a ball-and-socket joint represented by  $\Theta$ . The method of classification according to the proposed scheme is summarised as follows :—

All simple machine movements may be ranged in four divisions, viz. :—

1. Consisting of plane mechanisms, in which the pieces move in or parallel to the surface of a plane.
2. Spherical mechanisms, in which the pieces move in or parallel to the surface of a sphere.
3. Cylindrical mechanisms, in which the pieces move in or parallel to the surface of a cylinder.

4. The remainder, to which the name conoidal mechanisms is given, in which the axes of the swinging and turning motions neither meet nor are parallel.

The mechanisms in each of these divisions are classed in two sub-divisions.

Sub-division S, with surface contact of consecutive links.

Sub-division P, with point contact of consecutive links.

The mechanisms of sub-division S of divisions 1 and 2, 1, and 2, will consist of those in which O U I motions only are used.

Those of 3, will include the helical or V motion, and

Those of 4, will include the motion  $\ominus$  requiring the use of a ball-and-socket joint.

To the pairs of links which have the relative motions O, U, I, V, Reuleaux has given the name lower pairs. Reuleaux claimed two characteristics for lower pairs, viz. :—

1. Definiteness of motion derived from the surfaces of mutual contact themselves.

2. The possibility of distributing the contact over an area which may be extended as much as desired.

If it is desired to differentiate between the O and U motions, Reuleaux's turning pair cannot possess the first characteristic.

The second characteristic is of considerable value in relation to the liability to abrasion and wear, but the advantage of greater immunity against wear has to be purchased at the cost of a more complicated construction and a more restricted character of movement.

As examples—

The mechanism consisting of a pair of spur wheels turning in bearings which are at a fixed distance apart will belong to  $1_p$ .

A pair of bevel wheels will belong to  $2_p$ .

The so-called cylindrical cam motion will belong to  $3_p$ , and the worm-and-worm wheel mechanism to  $4_p$ .

The mechanisms in each of the eight sub-divisions are still further sub-divided into combinations. The combinations of  $1_p$ ,  $2_p$ , and  $3_p$ , are exhaustively enumerated, and it is suggested that an extension of the methods of applying the geometrical laws would lead to the preparation of an exhaustive list of the possible combinations in the other sub-divisions. The combinations are still further sub-divided into inversions according to Reuleaux's principle of the inversion of a machine.

Further than this there will be varieties of any inversion differing in the details of the construction and uses of the machine movement.

Lastly, the author proceeds to show how the foregoing considerations assist in the analysis of compound mechanisms. It is assumed

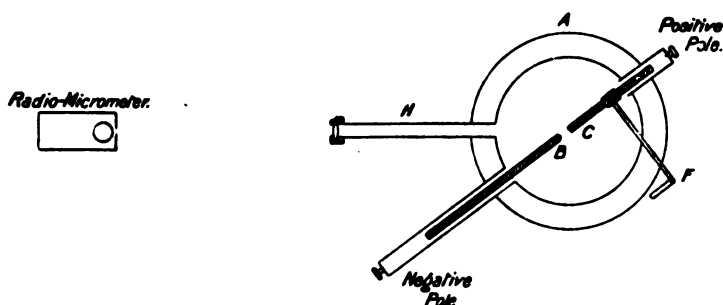
that practically all compound mechanisms contain a continuous mechanism A, of not more than four links, from which definiteness of relative motion of all the other links is derived. Any two links of A in their exact length, or longer or shorter, may be adapted to form with two new links a second mechanism B, and any two of A or B, or one of A and one of B, may be adapted to form with two still further added links a third mechanism C, and so on. In this way a definiteness of relative motion of many links in a compound mechanism is derived. The notation lends itself to a clear exhibition of the manner in which two or more simple mechanisms are associated together, and the compound mechanism built up.

III. "On the Effect of Pressure of the Surrounding Gas on the Temperature of the Crater of an Electric Arc Light. Preliminary Notes of Observations made at Daramona, Streete, co. Westmeath." By W. E. WILSON. Communicated by Professor FITZGERALD, F.R.S. Received April 25, 1895.

Of late years it has often been assumed that the temperature of the crater forming the positive pole of the electric arc is that of the boiling of carbon. The most modern determinations give this point as about  $3300^{\circ}$ — $3500^{\circ}$  C.

Solar physicists have thought that the photosphere of the sun consists of a layer of clouds formed of particles of solid carbon. At the temperature of these clouds is certainly not below  $8000^{\circ}$  C., it seems very difficult to explain how carbon can be boiling in the arc at  $3500^{\circ}$  and yet remain in the solid form in the sun at  $8000^{\circ}$ . Pressure in the solar atmosphere seemed to be the most likely cause of this, and yet, from other physical reasons, this seemed not probable.

In order to investigate whether increased pressure in the gas surrounding an electric arc would raise the temperature of the crater, I had an apparatus made by the Cambridge Instrument Company. It consists of a strong cast-iron box, which was tested by hydraulic pressure to 2000 lbs. per square inch. In the following plan, A is the box, B and C are the two carbon poles enclosed in steel tubes. The negative carbon was kept in position against a copper ring by a spiral spring behind it. The positive carbon was hand fed by a friction roller, which was moved by a handle F outside the box. A steel tube H was screwed into the box at such an angle that, by looking down it, we could see well into the crater of the positive pole. The end of this tube is closed by a glass lens, which formed an image of the crater at a distance of 80 cm.



A Boys's radio-micrometer, with its aperture reduced to about 2 mm. diameter, was so placed on the pier in the laboratory that the image of the crater fell on its small aperture. The instrument thus gave deflections proportional to the radiation coming from the crater. The current was supplied from a battery of accumulators, giving an E.M.F. of 110 volts. Suitable resistances of platinoid wire were put in the circuit, so that the current could be varied from 40 to 10 ampères. An ammeter was also in circuit, and the poles of the arc were connected to a voltmeter.

The gas used was nitrogen, and the pressure was got by connecting the box by a copper pipe with the valve of a 20-foot steel cylinder filled with the gas at a pressure of 120 atmospheres. A T-joint on the copper pipe was connected with a Bourdon pressure-gauge, which showed the pressure in the box at any moment.

The method of experimenting was first to start the arc with the pressure in the box at that of the atmosphere. The image of the brightest part of the crater was thrown on the aperture of the radio-micrometer, and a series of observations taken of the deflections of the instrument. The pressure was then gradually increased and the maximum deflections observed. As the pressure rises the resistance of the arc increases, and, in order to keep the same current flowing, the resistance in the circuit was reduced. It soon became evident that, even with moderate pressures of about 5 atmospheres, the temperature of the crater had fallen. This was not only shown by the reduction in the deflections of the radio-micrometer, but also by the fall in brilliancy of the image of the crater to the eye. The pressure was then increased to about 20 atmospheres, and the brilliancy of the crater fell to a dull red colour. These experiments were repeated several times and always with the same results.

I then tried the effect of reducing the pressure in the box by means of an air-pump, but as some of the glands in the box were only intended for an internal pressure, I found it impossible to get a good vacuum; yet by keeping the pump at work, and thus getting a

moderate vacuum, I found the radiation of the crater to be much greater than at the atmospheric pressure.

The temperature of the crater seemed very sensitive to any sudden diminution of pressure in the gas. If the blow-off valve was suddenly opened, the brilliancy of the crater fell so much that it became nearly invisible. When the box was being exhausted by the air-pump, although the temperature of the crater was rising as the vacuum improved, yet at each stroke of the pump the eye could see a distinct falling off of brilliancy in the image.

It was thought that the diminution of brilliancy might be due to smoke inside the box, but on looking through the window everything was seen sharply defined, also the gas as it issued from the blow-off was perfectly clear. The arc was also kept burning for some time in the box at the atmospheric pressure, but the image remained quite clear, and the inside of the box seemed quite free of smoke.

From these experiments it would seem as if the temperature of the crater, like that of a filament in an incandescent lamp, depends on how much it is cooled by the surrounding atmosphere, and not on its being the temperature at which the vapour of carbon has the same pressure as the surrounding atmosphere. That carbon volatilises in some form at comparatively low temperatures seems likely, from the way in which the carbon of incandescent lamp filaments is transferred to the glass. The pressure of the vapour of carbon in the arc may consequently be very small, and further it would seem that the supposition of high pressures in the solar photosphere, which has been referred to in the beginning of this paper, is not borne out by these experiments, and that carbon may exist there in the solid form at very high temperatures although the pressures are comparatively low.

The experiments on high pressures were conducted on several occasions. On the last occasion, in addition to repeated former experiments, the experiments on reduced pressures were performed, and I then had the great advantage of the presence, advice, and assistance of my friends Professor Minchin and Professor G. F. Fitzgerald. The later series of experiments entirely confirmed my former ones.



IV. "Note on the Motions\* of and within Molecules; and on the Significance of the Ratio of the two Specific Heats in Gases." By G. JOHNSTONE STONEY, M.A., D.Sc., F.R.S.  
Received April 29, 1895.

Take first a manifestly phosphorescent substance, such as calcium sulphide. For a long time after exposure to sunshine such a body retains the power of glowing in the dark, and the spectrum of the light which it then emits consists of definite lines or bands.

The temperature of the phosphorescent substance may all this time be low when tested by the thermometer, *i.e.*, by its power of communicating heat to bodies with which it is in contact.

Let us now apply another test of temperature. Imagine the phosphorescent body to be placed close outside one face of a closed chamber. Let the other sides of the chamber be adiabatic, while that next the phosphorescent substance is a window glazed with a material transparent to the phosphorescent rays, and opaque to all other radiations; and let there be a theoretically perfectly black solid within the chamber.

Such a solid emits all the rays due to its temperature, and them only—none others, however much a luminary may shine upon it.

Let us suppose that two specimens of the phosphorescent substance are provided, which can be alternately exposed to sunshine, and placed so as to radiate into the closed chamber through its window. In this way the phosphorescent radiation on to the black body within can be kept up for any desired length of time.

Let the black body at the commencement and the phosphorescent substance before its exposure to sunshine be at the ordinary atmospheric temperature. Now let the phosphorescent body be excited by sunshine, and put into its position in front of the window of the closed chamber. The light emitted by the phosphorescent substance will then radiate into the chamber without equivalent exchange. Accordingly the temperature of the black body within will rise, and will continue rising until its temperature has reached something like white heat: in fact until that point is reached at which a glowing black body radiates luminous rays which, at the particular parts of

\* The term motions as used in this paper is to be understood in a sense, both generalised and limited, as including along with the motions proper of ponderable matter, any other event of or within the molecule in which energy arriving from outside can be stored—whether in the form of potential energy of ponderable matter, or in motions or configurations of the electrons, or in any other way. It does not include any event which is *completely* isolated, if such exist within the molecule, *i.e.*, any event which is wholly incapable of exchanging energy with the other events going on within and outside the molecule.

the spectrum where the phosphorescent bands occur, are as bright as these latter. Then, and not till then, will an equivalent exchange take place of the energy passing in and out of the closed chamber, and only then will the augmentation of the temperature of the black body cease.

Accordingly, if we regarded only the temperature to which by radiation it can raise a neighbouring body, the phosphorescent substance would appear to be at a white heat.

This remarkable behaviour of the bodies that are conspicuously phosphorescent is, of course, consequent upon the molecular events that go on within them. We have accordingly next to inquire what their internal dynamical state must be to have such results.

We may distinguish the movements of molecules into three kinds of events.

1. A events; by which are to be understood motions of the centres of mass of the molecules relatively to one another. This is a kind of motion which is probably inconspicuous in solids, while considerable in liquids; and when we come to gases it becomes so much the predominant event that it usually engrosses about two-thirds of the energy of all the molecular events that are going on.

But besides these there may be motions of the parts of each molecule relatively to its centre of mass. These *internal events*, as they may be called, are of at least two kinds, as follows—

2. Ba events; *i.e.*, events within a molecule which act on and are reacted on by a neighbouring molecule if sufficiently near. Accordingly, whenever the Ba events within a molecule happen at any time to have more than average activity owing to the fluctuations in the distribution of energy that are always taking place, this excess of energy tends to be transferred over wholly or in part to the neighbouring molecules in consequence of the interaction between molecules. This interaction seems to proceed continuously in solids and liquids, but in gases only during a portion of the whole time, *viz.*, only on the occasions when encounters take place, which is perhaps something like one-seventieth of the whole time in the more perfect gases at atmospheric temperatures and pressures.

3. Bb events. But, besides, there may be more isolated events going on within molecules, events of the kind that manifest themselves conspicuously in phosphorescent bodies, events which but slowly part with any excess of energy they may possess to the other events going on within the molecule, or to neighbouring molecules.

It is to these Bb events that our attention has at present to be chiefly directed. A conspicuously phosphorescent substance was selected above in order most easily to demonstrate their existence. But what has been said of persistently phosphorescent bodies applies equally to many of the bodies about us which become visible when

illuminated. In fact many such bodies are evanescently phosphorescent,\* and the survival of the phosphorescence when the stimulating light has been withdrawn can be observed, and its duration measured, by the phosphoroscope. Although in most cases its duration is but a small fraction of a second, nevertheless in every case that has been observed, indeed in every case that can be observed with the phosphoroscope, it is a duration of immense length compared with the almost inconceivable rapidity of molecular events, in comparison with which even the thousandth part of one second is a vastly long period of time.†

In all such bodies therefore there are events of the class Bb.

Solids only seem to have been examined by the phosphoroscope. But we may feel assured that the same dynamical conditions prevail, certainly in liquids and probably in gases.

Let us next consider what bearing this has on the interpretation which is to be put on a high ratio of the two specific heats.

An event of the Bb class, which subsides so rapidly as to require the phosphoroscope to detect it, will behave, in any protracted experiment for determining the ratio of the two specific heats, in the same way as events of the Ba class. But this ceases to be the case where the ratio of the two specific heats is determined by experiments on sound; and in all the experiments which have been made use of it ceases to be the case when Bb events are as slow in subsiding as some of them are in conspicuously phosphorescent bodies.

Now the method by which the ratio of the two specific heats has been determined for argon and helium has been by experiments on sound; and as the value furnished by this method depends upon Ba events, it is competent to supply information about them only. It gives no information as to the energy involved in events of the Bb class. Accordingly it remains quite possible that Bb events may be easily evoked by stimulation of argon and helium, and that while in existence they may engross a considerable share of the total energy in the gas. That this is the case would seem to be evidenced by the vivid spectra which these gases exhibit under the influence of electricity.

\* The colours of objects, when not mere interference phenomena, are due to the excitation of Ba or Bb events within the molecules by certain rays of the incident light. In both cases the acting rays yield up their energy; but when Ba events take their place, the body is simply warmed: when it is Bb events that come into existence, the body for a short time subsequently radiates light. In the one case the colour of the body is that of incident light which is not absorbed; in the other case it is in general the colour of incident light which is absorbed. Of course if both causes are in operation they produce their conjoint effect. A few outlying cases, such as that of fluorescence, require a slightly modified treatment.

† The thousandth of a second bears about the same relation to molecular events that 10,000 years does to the motions of the limbs of animals.

In this connection, it is well to call to mind that phosphorescent events can be excited with even greater splendour by electricity than by exposure to light,\* as has been abundantly shown by experiment, especially by many experiments of exceptional brilliancy that have been made by Mr. Crookes.

There appears, therefore, to be no ground for the supposition which has been sometimes entertained, that there is incompatibility between the two facts that have been observed—the fact that in these two gases the ratio of the two specific heats is near its maximum value, and the fact that these gases, when stimulated by electricity, furnish brilliant spectra.

Events of the Bb class in phosphorescent bodies may be made to reach the intensity which enables them to emit visible radiations in any one of three ways, either—

1. By exposing the phosphorescent body to light of suitable wavelength and sufficient intensity; or
2. By exciting certain other electrical events in its neighbourhood;  
or
3. By raising the temperature of the whole phosphorescent body to a white heat.

If the phosphorescence is excited by either of the first two of these methods, the phosphorescent substance remains at a temperature as tested by the thermometer, immensely short of that which would enable an incandescent body to emit light of equally high refrangibility.

Accordingly the luminous effects within Geissler tubes do not prove that the temperature of the luminous gas is very high: an inference which is often erroneously drawn. In fact, Bb motions, when once excited within molecules, may continue for a considerable time to be more active [or, it may be, less active] than the Ba motions simultaneously going on, since there is but feeble interaction between them.

If the molecule consist of but one chemical atom, there may be both Ba and Bb events going on within that so-called atom. If the molecule consist of two or more chemical atoms, a part of the Ba events may be motions of the centres of mass of these atoms relatively to one another. But this is not always the case: the bonding between the atoms that form the molecule may be such that there is but little of this relative motion. Accordingly, the ratio of the two specific heats being large does not necessarily imply that the molecule is monatomic. The inference involves the erroneous supposition that there are no events going on within the molecule, and few

\* Or, rather, by electrical events other than light; since light is itself a manifestation of electricity.

degrees of freedom in its motion :\* a supposition which is an example of how ready we are to think that Nature must work simply when she works on a very small scale, and of the further error of imagining that a little rigid body is something exceptionally simple—the fact being that a rigid body is only a figment of the imagination, and that in Nature it is physically impossible. Moreover, it so happens that the bodies in Nature which most nearly resemble rigid bodies, namely, elastic solids, are amongst those bodies whose internal constitution is most complex.

Beside the Bb motions, there may be other internal events more or less isolated from both the Ba and the Bb events. By two events being isolated is to be understood their being unable to interchange energy. We may call these in succession Bc, Bd, &c., events. When they exist, the body will usually emit two or more spectra under variations of the external stimulus, whether luminous or electrical. And we must bear in mind that Ba events *may* also be the source of a spectrum.

The simplest supposition as to the interaction between the ether and the molecules of matter is that which is based on Faraday's law of electrolysis, which, as von Helmholtz pointed out, and as the present writer had previously shown, implies that there is a certain electrical charge, of the same amount in all cases, associated with each chemical bond (see 'Philosophical Magazine' for October, 1894, p. 418). The approximate amount of this charge, which Helmholtz designated the atom of electricity, and which the author has called the electron, can be computed. According to a determination made by the author in 1874, it appears to be about three-elevenths

\* That there are few degrees of freedom in the molecule is sometimes supposed to follow from the dynamical investigation ; but this appears to be a mistake. The Maxwell Law of the partition of kinetic energy is only known to prevail 1° where the kinetic energy is expressible as a sum of squares ; 2° where certain initial conditions of the motions of the system of bodies have been complied with ; and 3° where the subsequent events are due exclusively to the interaction of the bodies of the system.

No one of these is known to be true of any gas ; and the second of them if fulfilled initially will in general cease to prevail so soon as any agency other than the dynamical action of the molecules intervenes. Radiant heat, light, electricity, and many (probably all) chemical reactions, are agencies of this kind.

That the dynamical investigation, based on data simpler than those that prevail in nature, offers in a striking way an explanation of the numerical values for the ratio of the two specific heats as determined by experiment in several gases, in no degree proves that those simpler data are what exist in nature. Many and very various phenomena of light are explainable in a very striking way by the simple hypothesis that light is an undulation of transverse motions ; but it would be a rash inference to conclude from this that electromagnetic waves are mere transverse motions. The data of Nature have always to be simplified before they can be used as the data of mathematical investigations.

( $3 \times 10^{-11}$ ) of the C.G.S. electrostatic unit of quantity. Other estimates of the amount of this remarkable unit of electricity have since been made, and do not materially differ from the above. Here, then, we have all the machinery required. The motions which go on actively within chemical atoms can scarcely fail to wave about these electrons which are so closely associated with them; and we know that the waving about of such of these charges of electricity as for the time happen to be undisguised, must generate in the ether exactly such electro-magnetic waves as those revealed to us in the spectra of gases (see 'Transactions of the Royal Dublin Society,' vol. 4 (1891), p. 583).

- V. "On the Velocities of the Ions." By W. C. DAMPIER WHETHAM, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received May 2, 1895.

(Abstract.)

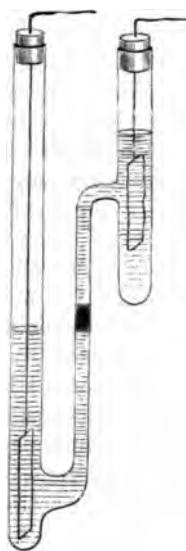
In a previous communication to the Royal Society ('Phil. Trans.,' 184, (1893), A. p. 337), I have described a method of experimentally determining the velocities of the ions during electrolysis, by observations on the phenomena which occur when a current of electricity is passed across the junction of two salt solutions, one at least of which is coloured.

The results obtained agreed, within the limits of experimental error, with the numbers deduced by Professor F. Kohlrausch from measurements of the conductivities. The method is, however, seriously restricted by the conditions necessary for its success. The two solutions must be of different densities, of different colours, and of nearly equal conductivities at equivalent concentrations.

In order to extend the method, I have used solid solutions in agar-agar jelly, tracing the motion of the ion by the formation of a precipitate. Jelly solutions were employed by Dr. Oliver Lodge, but, although he got a good result for hydrogen with phenol-phthallein as an indicator, when he tried to measure the velocity of barium and strontium by watching the formation of precipitates, the experiments were not very successful. This was probably due to the fact that, when a precipitate forms, it removes some of the electrolyte from solution, and so increases the specific resistance and the local potential-gradient. In order to eliminate this disturbing cause, instead of setting up two solutions which completely precipitate each other, like barium chloride and sodium sulphate, use was made of barium chloride and sodium chloride, just enough sodium sulphate being added to the latter to enable the motion of the barium ions to be traced by the

formation of a slight precipitate of barium sulphate. That this did not seriously affect the result was shown by making two determinations of the velocity of the barium ion, much more sodium sulphate being present in the first case than in the second. The numbers obtained for the specific ionic velocity were 0·000385 and 0·000390 cm. per second respectively.

The influence of the jelly was examined by determining the velocity of the bichromate group ( $\text{Cr}_2\text{O}_7$ ) with solid solutions of potassium bichromate and potassium chloride—the motion being indicated by the change in colour. The result was 0·00044 cm. per second. In the former paper the velocity of the same ion, measured by means of aqueous solutions of these salts, came out 0·00047 cm. per second. The influence of the jelly thus appears to be small, as is also indicated by Arrhenius' experiments on the conductivity of such solutions ('B. A. Report,' 1886, p. 344).



The apparatus used and the method of measurement were the same as in the former investigation. Two vertical glass tubes, about 2 cm. in diameter, were joined by a third, considerably narrower, which was bent parallel to the others for the greater part of its length. In this tube the jelly solutions came in contact with each other, and formed a slight precipitate at the junction. When a current was passed from one solution to the other, this precipitate spread, and the rate at which its advancing surface moved, was measured on a glass scale placed behind it, the observations being made through a telescope.

If the potential-gradient at the junction is  $dV/dx$ , we have

$$dV/dx = \gamma r/A,$$

where  $\gamma$  represents the total current,  $r$  the specific resistance of the solution, and  $A$  the area of cross-section of the tube.

If  $v$  is the observed velocity, the specific velocity for unit potential gradient is given by

$$v_1 = \frac{v}{dV/dx} = \frac{vA}{\gamma r}.$$

$A$  is determined by filling a known length of the tube with water or mercury,  $\gamma$  is read off on a galvanometer previously graduated by means of a Daniell cell and a box of resistance coils, and  $r$  is determined by Kohlrausch's method of a Wheatstone's bridge with alternating currents.

The solutions must be of nearly equal conductivities, so that a mean value of  $r$  may be taken. This is the more important because, unlike the colour-boundary method, the formation of a precipitate is an irreversible process. Measurements cannot, therefore, be made with the current flowing in both directions, which, in the former paper, was shown to get rid of the disturbing effect of any remaining difference in conductivity. All that can be done is to choose solutions whose conductivities are very nearly equal, so that the uncertainty which must appear in the result shall be, at all events, as small as possible.

The apparatus was immersed in a water bath, and the results all corrected to a temperature of  $18^\circ\text{C}$ ., in order that they might be comparable with Kohlrausch's calculated values.

The following results were obtained:—

*Barium*.—Solutions used: decinormal barium chloride and sodium chloride, a little sodium sulphate being added to the latter.

Temperature,  $15.8^\circ$ . Mean conductivity at  $15.8^\circ$  in reciprocals of legal ohms,  $9.60 \times 10^{-3}$ . Mean current,  $1.08/131$  ampère. Area of cross-section of tube,  $0.430$  sq. cm. Mean velocity of precipitate,  $0.446$  cm. in 10 minutes.

$$v_1 = vA/\gamma r = 0.000372 \text{ cm. per sec.}$$

The temperature coefficient was found to be  $2.5$  per cent. per degree, so that we get for the specific ionic velocity of the barium ion, travelling through a decinormal solution of barium chloride in solid agar jelly at a temperature of  $18^\circ$ ,

$$v_{\text{Ba}} = 0.000393 \text{ cm. per sec.}$$

For an aqueous solution of this strength Kohlrausch gives ('Wied. Ann.,' vol. 50, p. 385)

$$v_{\text{Ba}} = 0.000366 \text{ cm. per sec.}$$

With new solutions, containing only just enough sulphate to give a visible precipitate, the result was

$$v_{Ba} = 0.000386 \text{ cm. per sec.}$$

*Calcium.*—Solutions used: decinormal calcium chloride and sodium chloride, the latter containing a considerable amount of sodium carbonate, in order to get a visible precipitate; this increases the disturbing effect of the precipitation.

Temperature,  $18.1^{\circ}$ . Mean conductivity at  $18.1^{\circ}$ ,  $8.91 \times 10^{-3}$ .

Mean current, 1.08/153 ampère. Area, 0.442 sq. cm. Mean velocity, 0.376 cm. in 10 minutes.

$$v_{Ca} = 0.000349 \text{ cm. per sec.}$$

Kohlrausch gives  $v_{Ca} = 0.000290 \text{ cm. per sec.}$

*Silver.*—Solutions used: decinormal silver nitrate and sodium nitrate, the latter containing a little sodium chloride.

Temperature,  $17.4^{\circ}$ . Mean conductivity at  $17.4^{\circ}$ ,  $8.96 \times 10^{-3}$ . Mean current, 1.08/164 ampère. Area, 0.442 sq. cm. Mean velocity, 0.480 cm. in 10 minutes.

$$v_{Ag} = 0.000488 \text{ cm. per sec.}$$

Kohlrausch gives  $v_{Ag} = 0.000462 \text{ cm. per sec.}$

*The Sulphate Group (SO<sub>4</sub>).*—Solutions used: decinormal sodium sulphate and sodium chloride, the latter containing a little barium chloride.

Temperature,  $15.2^{\circ}$ . Mean conductivity at  $15.2^{\circ}$ ,  $9.69 \times 10^{-3}$ . Mean current, 1.08/246. Area, 0.430 sq. cm. Mean velocity, 0.257 cm. in 10 minutes.

$$v_{So_4} = 0.000434 \text{ cm. per sec.}$$

Another determination in a tube whose area of cross-section was 0.746 sq. cm. gave

$$v_{So_4} = 0.000458 \text{ cm. per sec.}$$

Kohlrausch gives  $v_{So_4} = 0.000492 \text{ cm. per sec.}$

The general result goes to show that the ionic velocities thus measured agree, within the limits of experimental error, with Kohlrausch's numbers.\*

It has already been shown ('Phil. Mag.,' October, 1894) that when travelling through acetates whose concentration is 0.07 normal, the velocity of the hydrogen ion is about 0.000065 cm. per second,

\* It is worthy of note that all the results for kations are slightly larger than indicated by theory, while the only measurement made for an anion gives a value which is slightly less. This may possibly be a result of the use of jelly.

whereas in other solutions, such as chlorides, it is about 0.0030, that is, about 46 times as great. Now acetic acid at the concentration mentioned above has an abnormally low conductivity, only the 1/62 part of that of an equivalent solution of hydrochloric acid, so that, in such cases, the *immediate* cause of the low conductivity appears to be a reduction in the ionic velocities.

An attempt was made to complete the investigation of acetic acid by measuring the velocity of the acetate group  $C_2H_3O_2$ . I thought the red colour, which acetates give with ferric salts, might be used as indicator, and for this purpose set up solutions of ferric chloride and ferric chloride coloured red by ferric acetate. These ferric salts are said to be decomposed in solution into ferric hydroxide and the acid. Besides the chemical reasons in favour of this hypothesis, it is supported by the conductivities. Ferric chloride, which gives hydrochloric acid, is known to have abnormally great conductivity, and measurements I have carried to great dilution show that the molecular conductivity reaches a maximum at a certain concentration, and, as the dilution is pushed further, sinks again. This behaviour is characteristic of the solutions of acids. In the case of ferric acetate, acetic acid is produced, and the molecular conductivity is abnormally low. It seemed likely, then, that the red colour produced by acetates, when added to solutions of ferric salts, might be used as a means of measuring the velocity of the acetate group in acid solutions. When the experiment was made, however, it was found that the colour boundary travelled in the wrong direction for an anion, viz., with the current, the specific velocity being 0.00028 cm. per second. Now it is unlikely that an ion should behave thus, and an experiment on the migration phenomena of a solution of acetic acid showed that there was no accumulation of acid round the kathode. The result of further investigation was to show that the red colour of such solutions is due to the presence of soluble ferric hydroxide, and that, under the influence of a current, this is transported through the solution without decomposition in the direction of the current. Among other experiments, a direct measurement of the velocity of the transport was made. If a solution of ferric chloride be dialysed through parchment paper, hydrochloric acid escapes, while a red solution of soluble ferric hydroxide, known as "dialysed iron," remains. This was used to colour a solution of ferric chloride, which was set up in contact with an ordinary aqueous solution of ferric chloride of the same concentration. The specific velocity of the hydroxide could thus be determined by observing the motion of the colour boundary, and came out 0.00033 cm. per second in the direction of the current. It is evident that this is what we were measuring in the case of the acetate described above.

The conductivity of the dialysed iron solution is very low, and an

investigation on its value for solutions of different concentration led to the conclusion that, in such solutions, the whole work of carrying the current is done by the residual ferric chloride, which is itself probably decomposed to some extent into hydroxide and acid, though perhaps the proportion decomposed is not so large as in solutions without an excess of ferric hydroxide, which is one of the products of the decomposition.

An experiment on the migration of acetic acid showed that the velocity of the acetate group ( $C_2H_3O_2$ ) was, at all events, very small, so that, as in the case of mineral acids, the conductivity is mainly due to the motion of the hydrogen.

The following table gives the velocities of all ions which have been experimentally determined :—

| Name of ion.                     | Specific ionic velocity in centimetres per second. |                    | Observer.         |
|----------------------------------|----------------------------------------------------|--------------------|-------------------|
|                                  | Calculated from Kohlrausch's theory.               | Directly observed. |                   |
| Hydrogen (in chlorides) ...      | 0·0028                                             | 0·0026             | O. J. Lodge.      |
| " (in acetates) ....             | 0·000048                                           | 0·000065           | W. C. D. Whetham. |
| Copper .....                     | —                                                  | 0·00031            | "                 |
| Bichromate group ( $Cr_2O_7$ ).. | 0·00047                                            | 0·00047            | "                 |
| Barium .....                     | 0·00037                                            | 0·00039            | "                 |
| Calcium .....                    | 0·00029                                            | 0·00035            | "                 |
| Silver .....                     | 0·00046                                            | 0·00049            | "                 |
| Sulphate group ( $SO_4$ ).....   | 0·00049                                            | 0·00045            | "                 |
| Cobalt in alcoholic $CoCl_2$ ..  | —                                                  | 0·000022           | "                 |
| " " $Co(NO_3)_2$ ..              | —                                                  | 0·000044           | "                 |
| Chlorine " $CoCl_2$ ...          | —                                                  | 0·000026           | "                 |
| Nitrate group " $Co(NO_3)_2$ ..  | —                                                  | 0·000035           | "                 |

The values are calculated from Kohlrausch's theory for the same strength of solution as that used for the direct observation. In the case of copper, in decinormal copper chloride solution, there are no migration data for this. The velocity of the copper ion *at infinite dilution* is given by Kohlrausch as 0·00031. The sum of the ionic velocities of cobalt nitrate in alcohol, as calculated from the conductivity, comes out 0·000079, and that for cobalt chloride 0·000060. These numbers are to be compared with the sum of the observed velocities given above, namely, 0·000079 and 0·000048 respectively.

The Society adjourned over the Whitsuntide Recess to Thursday, June 13.

*Presents, May 30, 1895.*

## Transactions.

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- Belgrade:—Royal Servian Academy. Glas. 46, 47. [*Servian.*] 8vo. *Belgrade* 1895; Spomenik. 28. [*Servian.*] 4to. *Belgrade* 1895. The Academy.
- Cambridge, Mass.:—Museum of Comparative Zoölogy. Bulletin. Vol. XVI. No. 15. Vol. XXVI. No. 2. 8vo. *Cambridge, Mass.* 1895. The Museum.
- Coimbra:—Universidade. Anuario. 1894-95. 8vo. *Coimbra* 1895. The University.
- Kazan:—Imperial University. Ucheniia Zapiski [*Scientific Notes.*] 1895. No. 4. 8vo. *Kazan.* The University.
- Lausanne:—Société Vaudoise des Sciences Naturelles. Bulletin. Vol. XXX. No. 116. 8vo. *Lausanne* 1894. The Society. -
- London:—British Museum. Catalogue of Additions to the Manuscripts in the Years 1888-93. 8vo. *London* 1894. The Trustees. -
- Geologists' Association. Proceedings. Vol. XIV. Part 2. 8vo. *London* 1895. The Association. -
- Mineralogical Society. The Mineralogical Magazine. Vol. XI. No. 49. 8vo. *London* 1895. The Society. -
- Odontological Society. Transactions. Vol. XXVII. No. 6. 8vo. *London* 1895. The Society. -
- Royal Meteorological Society. Quarterly Journal. Vol. XXI. No. 94. 8vo. *London* 1895; The Meteorological Record. Vol. XIV. No. 55. 8vo. *London.* The Society. -
- Royal United Service Institution. Journal. May, 1895. 8vo. *London.* The Institution. -
- Manchester:—Manchester Geological Society. Transactions. Vol. XXIII. Parts 5-7. 8vo. *Manchester* 1895. The Society. -
- Naples:—Accademia delle Scienze Fisiche e Matematiche. Rendiconto. Anno XXXIV. Fasc. 4. 8vo. *Napoli* 1895. The Academy.
- Paris:—Comité International Permanent pour l'Exécution Photographique de la Carte du Ciel. Bulletin. Tome II. Fasc. 3. 4to. *Paris* 1895. Académie des Sciences, Paris.
- École Normale Supérieure. Annales Scientifiques. Tome XII. No. 5. 4to. *Paris* 1895. The School.
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**Transactions** (*continued*).

- Société de Géographie. Bulletin. Tome XV. Trim. 4. 8vo. *Paris* 1894. The Society.
- Société Mathématique de France. Bulletin. Tome XXIII. Nos. 2, 3. 8vo. *Paris* [1895]. The Society.
- Prague:—Königl. Böhm. Gesellschaft der Wissenschaften. Sitzungsberichte. 1894. 8vo. *Prag* 1895; Jahresbericht. 1894. 8vo. *Prag* 1895. The Society.
- Southport:—Society of Natural Science. Report. 1891-93. 8vo. *Southport* 1895. The Society.
- Tokio:—Kais.-Japanische Universität. Mittheilungen aus der Medicinischen Facultät. Bd. III. No. 1. 4to. *Tokio* 1894. The University.
- Toulouse:—Faculté des Sciences. Annales. Tome IX. Fasc. 2. 4to. *Paris* 1895. The Faculty.
- Vienna:—Kais. Akademie der Wissenschaften. Sitzungsberichte (Math.-naturw. Classe). Bd. CIV. Abth. 1. Heft 1, 2. 8vo. *Wien* 1895. The Academy.

**Observations and Reports.**

- Calcutta:—Meteorological Department, Government of India. Indian Meteorological Memoirs. Vol. VII. Parts 1, 2. Folio. *Simla* 1894. The Department.
- London:—Meteorological Office. Meteorological Charts of the Red Sea. Folio. *London* 1895. The Office.
- Melbourne:—Department of Mines. Reports on the Victorian Coal-fields (No. 3). 4to. *Melbourne* 1895. The Department.
- Paris:—Observatoire. Rapport Annuel. 1894. 4to. *Paris* 1895. The Observatory.
- San Fernando:—Instituto y Observatorio de Marina. Observaciones Meteorológicas y Magnéticas. Año 1893. 4to. *San Fernando* 1894. The Institute.
- Washington:—U.S. Department of Agriculture. Experiment Station Record. Vol. VI. No. 7. 8vo. *Washington* 1895. The Department.
- Weather Bureau. Report of the Ohio Weather and Crop Service. March, 1895. 8vo. *Columbus, O.* 1895. The Bureau.

**Journals.**

- Archives des Sciences Biologiques. Tome III. No. 4. 8vo. *St. Pétersbourg* 1895.
- Institut Impérial de Médecine Expérimentale, St. Petersburg.

*Journals (continued).*

- Revista de Chihuahua.* Año I. No. 3. 8vo. *Chihuahua* 1895.  
The Editor.
- Scientific Roll and Magazine of Systematized Notes.* Climate:  
Baric Condition. Nos. 6, 7. 8vo. *London* 1894-95.  
Mr. A. Ramsay.
- Zeitschrift für Naturwissenschaften.* Bd. LXVII. Heft 6. 8vo.  
*Leipzig* 1894.  
Naturw. Verein für Sachsen und Thüringen, Halle a. S.
- 

- Campbell (F.) *The Bibliography of the Future: a Paper reviewing  
the existing Condition of National and International Biblio-  
graphy, with suggested Reforms.* 8vo. *London* 1895.  
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- Courmelles (F. de) *L'Électricité Curative.* 8vo. *Paris* 1895.  
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- Lamb (H.), F.R.S. *Hydrodynamics.* 8vo. *Cambridge* 1895.  
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- Maltese (F.) *Le Ambliopie curate per la Via Dentaria.* 8vo. *Napoli*  
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- Roscoe (Sir H. E.), F.R.S. *John Dalton and the Rise of Modern  
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*June 13, 1895.*

Annual Meeting for the Election of Fellows was held this day.

DRD KELVIN, D.C.L., LL.D., President, in the Chair.

Minutes relating to the election of Fellows having been read, and Mr. R. H. Scott were, with the consent of the Society, Scrutators to assist the Secretaries in examining the lists.

Names of the Fellows present were then collected, and the following candidates were declared duly elected into the Society :—

|                            |                                  |
|----------------------------|----------------------------------|
| Wolfe, C.B.                | Holden, Major Henry Capel Lofft, |
| Professor Alfred Gibbs,    | R.A.                             |
| George Hartley, M.A.       | McClean, Frank, M.A., LL.D.      |
| John, M.A.                 | MacEwen, Professor William,      |
| Professor Joseph Reynolds, | M.D.                             |
| Ernest Howard, M.A.        | Martin, Sidney, M.D.             |
| Charles Thomas, M.A.       | Minchin, Professor George M.,    |
| Professor Sydney John,     | M.A.                             |
|                            | Power, William Henry.            |
|                            | Purdie, Professor Thomas, B.Sc.  |

were given to the Scrutators.

June 13, 1895.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

His Royal Highness the Duke of Saxe-Coburg-Gotha (elected March 16, 1882) was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "On the new Gas obtained from Uraninite. Fourth Note."  
By J. NORMAN LOCKYER, C.B., F.R.S. Received May 28, 1895.

Continued experiments on the gases obtained by heating the minerals bröggerite and euxenite *in vacuo* have revealed the presence in the spectrum of an important line in the infra-red. By comparisons with the solar spectrum in the first order grating spectrum, the wave-length of the line has been approximately determined as 7065. There can be little doubt, from the observations which have been made, that this new line is coincident with a chromospheric line which occurs in Young's list, having a frequency of 100, of which the wave-length on Rowland's scale is stated to be 7065·5.

It follows therefore that, besides the hydrogen lines, all three chromospheric lines in Young's list which have a frequency of 100 have now been recorded in the spectra of the new gas or gases obtained from minerals by the distillation method.

These are as follows :—

7065·5  
5875·98  
4471·8

The wave-lengths of the lines are in Rowland's scale, as given in Scheiner's "Astronomical Spectroscopy."\* In a partial revision of his chromospheric list, Professor Young gives the corona line 5316·79 as also having a frequency of 100 in the chromosphere, but, up to the present, this line has not been observed in the laboratory.

\* Frost's translation, p. 184.

## II. "On the New Gas obtained from Uraninite. Fifth Note."

By J. NORMAN LOCKYER, C.B., F.R.S. Received May 29, 1895.

In a former communication I pointed out the spectroscopic evidence, furnished by the isolation of lines in certain minerals, which indicates that the complete spectrum obtained when bröggerite is submitted to the distillation method is produced by a mixture of gases.

In order to test this view, I have recently made some observations, based on the following considerations.

(1.) In a simple gas like hydrogen, when the tension of the electric current given by an induction coil is increased, by inserting first a jar, and then an air-break into the circuit, the effect is to increase the brilliancy and the breadth of all the lines, the brilliancy and breadth being greatest when the longest air-break is used.

(2.) Contrariwise, when we are dealing with a known compound gas; at the lowest tension we may get the complete spectrum of the compound without any trace of its constituents, and we may then, by increasing the tension, gradually bring in the lines of the constituents, until, when complete dissociation is finally reached, the spectrum of the compound itself disappears.

Working on these lines, the spectrum of the spark at atmospheric pressure, passing through the gas, or gases, distilled from bröggerite, has been studied with reference to the special lines C (hydrogen), D<sub>5</sub>, 667, and 447.

The first result is that all the lines do not vary equally, as they should do if we were dealing with a simple gas.

The second result is that at the lowest tension 667 is relatively more brilliant than the other lines; on increasing the tension, C and D<sub>5</sub>, considerably increase their brilliancy, 667 relatively and absolutely becoming more feeble, while 447, seen easily as a narrow line at low tension, is almost broadened out into invisibility as the tension is increased in some of the tubes, or is greatly brightened as well as broadened in others (fig. 1).

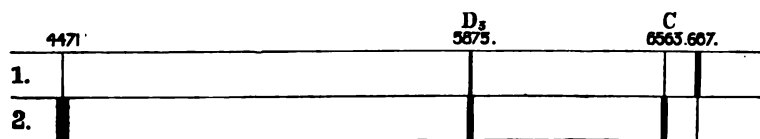


FIG. 1.—Diagram showing changes in intensities of lines brought about by varying the tension of the spark. 1. Without air-break. 2. With air-break.

The above observations were made with a battery of five Grove cells; the reduction of cells from 5 to 2 made no difference in the phenomena except in reducing their brilliancy.

Reasoning from the above observations, it seems evident that the effect of the higher tension is to break up a compound, or compounds, of which C, D<sub>3</sub> and 447 represent constituent elements; while, at the same time, it would appear that 667 represents a line of some compound which is simultaneously dissociated.

The unequal behaviour of the lines has been further noted in another experiment, in which the products of distillation of bröggerite were observed in a vacuum tube and photographed at various stages. After the first heating, D<sub>3</sub> and 4471 were seen bright, before any lines other than those of carbon and hydrogen made their appearance. With continued heating, 667, 5016, and 492 also appeared, although there was no notable increase of brightness in the yellow line; still further heating introduced additional lines 5048 and 6347.

These changes are represented graphically in the following diagram (fig. 2).

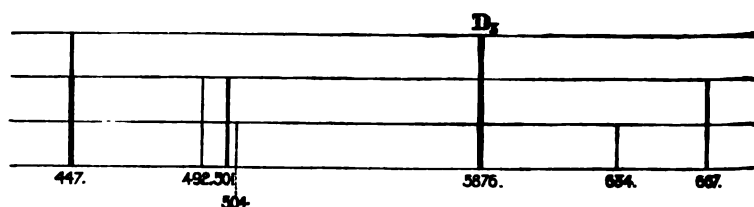


FIG. 2.—Diagram showing order in which lines appear in spectrum of vacuum tube when bröggerite is heated.

It was recorded further that the yellow line was at times dimmed, while the other lines were brightened.

In my second note, communicated to the Royal Society on the 8th instant, I stated that I had never once seen the lines recorded by Thalén in the blue, at  $\lambda$  4922 and 4715.

It now seems possible that their absence from my previous tubes was due to the fact that the heating of the minerals was not sufficiently prolonged to bring out the gases producing these lines.

It is perhaps to the similar high complexity of the gas obtained from clèveite that the curious behaviour of a tube which Professor Ramsay was so good as to send me, must be ascribed. When I received it from him, the glorious yellow effulgence of the capillary, while the current was passing, was a sight to see. But after this had gone on for some time, while the coincidence of the yellow line with D<sub>3</sub> of the chromosphere was being inquired into, the luminosity of the tube was considerably reduced, and the colours in the capillary

and near the poles were changed. From the capillary there was but a feeble glimmer, not of an orange tint, while the orange tint was now observed near the poles, the poles themselves being obscured by a coating on the glass of brilliant metallic lustre.

After attempting in vain for some time to determine the cause of the inversion of D, and 447 in various photographs I had obtained of the spectra of the products of distillation of many minerals, it struck me that these results might be associated with the phenomena exhibited by the tube, and that one explanation would be rendered more probable if it could be shown that the change in the illumination of the tube was due to the formation of platinum compounds, platinum poles being used. On May 21st I accordingly passed the current and heated one of the poles, rapidly changing its direction to assure the action of the negative pole, when the capillary shortly gave a very strong spectrum of hydrogen, both lines and structure. A gentle heat was continued for some time and apparently the pressure in the tube varied very considerably, for as it cooled the hydrogen disappeared and the D<sub>2</sub> line shone out with its pristine brilliancy. The experiment was repeated on May 24th and similar phenomena were observed.

III. "Further Observations on the Organisation of the Fossil Plants of the Coal-measures. Part III. *Lyginodendron* and *Heterangium*." By W. C. WILLIAMSON, LL.D., F.R.S., Emeritus Professor of Botany in the Owens College, Manchester, and D. H. SCOTT, M.A., Ph.D., F.R.S., Honorary Keeper of the Jodrell Laboratory, Royal Gardens, Kew. Received May 14, 1895.

(Abstract.)

*Introduction.*

The two genera, *Lyginodendron* and *Heterangium*, are among the most interesting and at the same time the most puzzling representatives of the Carboniferous flora. Although we are still without any satisfactory evidence as to the reproductive organs in either genus, yet the organisation of their vegetative members is preserved with such completeness and perfection as to show that these fossils present a combination of characters such as exists in no living group of plants.

The evidence afforded by the vegetative characters clearly points to a position intermediate between ferns and Cycadææ.

I. *LYGINODENDRON*.

*Lyginodendron oldhamium*, Will.,\* is one of the commonest fossils preserved in the calcareous nodules of the Lancashire and Yorkshire coal-measures, and has also been found in those of Germany and Austria. A renewed investigation, with the aid of numerous additional specimens, has enabled us to clear up many doubtful points in the structure of the plant, and to give for the first time a complete account of all its vegetative organs.

A. *The Stem.*

1. *General Structure.*—The middle of the central cylinder or stele is occupied by a parenchymatous pith. Surrounding this is the primary wood, which usually forms a ring of from five to eight distinct strands. Beyond this we find, in all but the youngest specimens, a broad zone of secondary wood, then the cambium, and next the phloëm. The whole stele is bounded by a well-marked pericycle. The inner cortex is mainly parenchymatous, while the outer zone consists of alternating strands of fibres and parenchyma, constituting the well-known "dictyoxylon cortex" of Count Solms-Laubach.

The pericycle and cortex are traversed by the leaf-trace bundles, which alternate with the perimedullary xylem-strands.

2. *Course of the Vascular Bundles.*—We have obtained direct proof that the perimedullary strands of xylem form the downward continuation of the bundles which pass out into the leaves. Thus the entire bundle-system of the stem is built up of the leaf-traces. Each leaf-trace extends through at least ten internodes; five internodes are traversed while it is passing through cortex and pericycle, and five more after it has reached the periphery of the pith. On entering the pith the trace turns aside in the kathodic direction, and unites with the adjacent perimedullary strand on that side. We thus see that these strands are sympodial bundles, made up of the united lower portions of adjacent leaf-traces.

In the upper part of its course, each leaf-trace consists of two bundles, which unite into one in passing through the pericycle.

The phyllotaxis was usually two-fifths, but in the smallest stems was probably one-third.

3. *Structure of the Vascular Bundles.*—The preservation is so good that we have been able to determine with certainty that the bundles in the stem were normally *collateral*, having xylem on their inner, and phloëm on their outer side. As they passed out into the leaves

\* See Williamson, "Organisation of the Fossil Plants of the Coal-measures. Part IV," 'Phil. Trans.,' 1873; Part VI, 'Phil. Trans.,' 1874; Part VII, 'Phil. Trans.,' 1876; Part XIII, 'Phil. Trans.,' 1887; Part XVII, 'Phil. Trans.,' 1890.

their structure became *concentric*, the phloëm here extending all round the xylem.

The xylem of the bundles in the *stem* of *Lyginodendron* exactly resembles that in the *leaves* of existing Cycadæ. The protoxylem lies in the interior of the primary wood, but near its outer side, so that the greater part of the primary wood was centripetally developed, while a smaller portion was centrifugal. We propose to term such bundles *mesoxylie* or *mesarch*.\* All statements as to the position of the protoxylem are based on longitudinal as well as on transverse sections.

4. *The Secondary Tissues*.—A few young stems have been observed with little or no secondary thickening; in most specimens it has made considerable progress. A large amount of secondary wood and bast, both fascicular and interfascicular, was formed, by means of a normal cambial layer, which is often well preserved.

The tracheides of the wood have numerous bordered pits on their radial surfaces. Similar elements occur in the primary wood also. The rows of tracheides are separated by numerous medullary rays.

The phloëm is often well preserved, so that primary and secondary phloëm can be distinguished.

The secondary tissues bear a general resemblance to those in the stems of Cycadæ.

5. *Pith and Pericycle*.—Both these tissues contained nests of dark coloured elements, probably of a sclerotic nature. They are also traversed by numerous rows of cells with carbonaceous contents, which may have been secretory *sacs*, but not intercellular *canals*. At the outer border of the pericycle a characteristic internal *periderm* was developed.

6. *The Cortex*.—The parenchymatous portions of the outer cortex became much dilated in the older stems, in consequence of the secondary growth.

7. *On Small Stems of the Lyginodendron Type*.—Certain very small stems have been described, differing in structure from the usual form. In some of these the primary xylem forms a continuous ring, instead of being divided into distinct bundles. We now suggest that these specimens may represent the *basal*, first-developed region, of normal stems. In *Osmunda*, which in many respects resembles *Lyginodendron*, it has been shown by M. Leclerc du Sablon, that the embryonic stem has the same peculiarity.

8. *Structural Anomalies*.—Some of the specimens show remarkable individual anomalies, the most frequent and conspicuous of which consists in the appearance of a cambium at the periphery of the pith,

\* One of the authors has recently found that this peculiarity sometimes extends to *stem-structures* in Cycadæ; in the peduncles of both male and female flowers of *Stangeria* the bundles are often mesoxylie.

forming medullary wood and bast, with inverted orientation. This is precisely the anomaly shown by certain species of *Tecoma*, and other dicotyledons. The anomalous medullary cambium is continuous with the normal cambium through the leaf-trace gaps. This case is a striking instance of the independent appearance of the same structural peculiarity in families as remote as possible from each other.

### B. The Leaf.

1. *Connexion between Leaf and Stem*.—New and conclusive evidence has been found, confirming the conclusion previously arrived at (in Mem. XVII), that "*Rachiopteris aspera*" is the petiole of *Lyginodendron*. In several specimens petioles with the characteristic structure of that fossil, are found inserted on the stems of *Lyginodendron*. The vascular bundles on leaving the pericycle of the stem bend out rapidly into the base of the leaf, becoming concentric at the same time. Petioles, continuous with the *Lyginodendron* stem, have been traced up to the point where they begin to ramify.

2. *Form of the Leaf*.—The petioles, which we now know to belong to our plant, branch repeatedly, and ultimately give rise to small palmately-segmented leaflets. The leaf was thus a highly compound one, and we can confirm the statement previously made, that the character of the foliage was that of Brongniart's form-genus *Sphenopteris*.

3. *Structure of the Petiole*.—The most important point here is that throughout the petiole and rachis, the vascular bundles, of which either one or two are present, are of typical concentric structure, as in a fern.

The cortex of the petiole has essentially the same structure as that of the stem.

4. *Structure of the Lamina*.—We have examined sections of leaflets (found in connexion with petioles of *Lyginodendron*), in which the structure is perfectly preserved. The lamina had a distinctly bifacial structure, with well-differentiated palisade, and spongy parenchyma. Stomata have only been observed on the lower surface. The vascular bundles in the lamina appear to have been collateral, as is also the case in recent ferns.

5. *On a Peculiar Bud-like Structure*.—This is a unique specimen, consisting of an axis, of obscure structure, bearing numerous appendages which exactly resemble the well-known cortical outgrowths of the stem and petiole of *Lyginodendron*. At first sight, the specimen bears some resemblance to a cone, but it was more probably a bud or young leaf, from which the inner delicate tissues have perished, leaving the protective outer coat, bearing the appendages, which may represent the bases of paleæ.

## C. The Root.

1. *Connexion between Root and Stem.*—We have already briefly recorded our discovery that "*Kaloxylon Hookeri*" is the root of *Lyginodendron*.<sup>\*</sup> We have found that certain appendages of the stem of *Lyginodendron*, most of which were formerly described as "branches," are in reality of endogenous origin, as is shown by the fact that the appendage, in passing through the cortex of the parent stem, is surrounded by a well-defined cortex of its own. These appendages are further shown to be roots, by the structure of their central cylinder and their mode of branching. Sections of the free part of the same organs, which are in connexion with stems of *Lyginodendron*, show that they agree in all respects with "*Kaloxylon Hookeri*," namely, in the structure and arrangement of both primary and secondary wood, and in the details of the cortex, which is well characterised by its double or treble external or epidermal layer.

We find then that the stem of *Lyginodendron* bore numerous adventitious roots, of endogenous origin, and that these roots are identical with the fossils previously described under the name of "*Kaloxylon Hookeri*."

2. *Primary Structure of the Root.*—All the specimens of "*Kaloxylon Hookeri*" have been re-examined, and are found to present a perfectly typical root-structure. The stele varies from triarch to octarch structure in different specimens. The protoxylem is external, showing centripetal development of the primary wood. In favourable specimens the regular alternation of the phloëm-groups with those of xylem is quite clear. The stele has no pith, but there is a considerable amount of conjunctive parenchyma. Both pericycle and endodermis are present. The inner cortex contains abundant "secretory sacs."

The young roots much resemble the smaller adventitious roots of *Marattiaceæ*.

3. *Secondary Tissues of the Root.*—These are beautifully preserved, and are found at all stages of development. The cambium is often specially clear. Secondary growth began opposite the phloëm-groups, and the secondary wood is generally interrupted by large rays opposite the protoxylem-strands. The secondary tissues resemble those of the stem. The whole process of secondary growth was perfectly normal, as in dicotyledons at the present day.

4. *Branching of the Root.*—The numerous specimens showing branching prove that the rootlets were endogenous, and that they arose opposite the protoxylem-groups of the main root.

<sup>\*</sup> 'Roy. Soc. Proc.,' vol. 56, 1894.

D. *Habit and Dimensions of the Plant.*

In none of our authentic specimens is the stem more than 4 cm. in thickness. Certain cortical impressions, belonging to much larger stems, have been referred to *Lyginodendron*, but on inconclusive grounds.

There is one large specimen showing structure, in which only the secondary wood and portions of the pith are preserved. So much of the structure as remains agrees closely with that of *Lyginodendron*. This specimen may have reached a diameter of 30 cm. or 40 cm., and establishes a certain probability that *L. Oldhamium*, or some allied species, may have attained the dimensions of a small tree.

The ordinary specimens must have had upright stems of considerable height, bearing spirally-arranged, compound, fern-like leaves, separated by internodes about an inch long. The lower parts of the stem gave off on all sides numerous adventitious roots.

The entire absence of fructification is remarkable, considering the great frequency and admirable preservation of our fossil. It may be explained, either on the hypothesis that the leaves bore very caducous, fern-like sporangia, or by supposing that our material consists entirely of immature specimens.

II. *HETERANGIUM.**Introduction.*

The genus *Heterangium* differs conspicuously from *Lyginodendron*, in the structure of the stele of the stem, which in *Heterangium* contains no pith, but has a solid axis of primary wood. In most other respects the two genera much resemble each other.

i. *Heterangium Grievii*, Will.\*

The original specimens of this species were derived from the Burntisland deposits. At a later date, specimens were found in the coal-measures of Dulesgate, Lancashire, which have been referred to the same species, though they show some slight differences from the original form.

A. *The Stem.*

1. *General Structure.*—The whole interior of the stele is occupied by the primary wood, consisting of tracheides intermixed with conjunctive parenchyma. In most specimens a certain amount of secondary wood has been formed around the central mass. Outside

\* Williamson, 'British Association Reports,' 1871; "Organisation," Part IV, 1872; Part XVII, 1890.

the wood a zone of phloëm can be traced, and this again is surrounded by a parenchymatous belt, which we regard as pericycle. The inner cortex is characterised by the presence of horizontal plates of sclerotic tissue. The outer cortical zone has a structure similar to that of *Lyginodendron*. In the pericycle and cortex numerous leaf-trace bundles are met with.

2. *Course of the Vascular Bundles*.—The bundles can be traced from the stele into the bases of the leaves. Their arrangement indicates that the phyllotaxis was three-eighths in the larger and two-fifths in the smaller stems. Each leaf received a single bundle. The leaf-trace bundles can be followed downwards for some distance at the periphery of the stele, where they form distinct strands, though united with the axial wood.

3. *Primary Structure of the Stele and Leaf-trace Bundles*.—The strands at the periphery of the stele, as well as the leaf-trace bundles with which they are continuous, have the same collateral and mesoxylic structure as the bundles in the stem of *Lyginodendron*, or the foliar bundles of Cycadææ. The essential difference from *Lyginodendron* consists in the fact that in *Heterangium* these bundles are united by the axial xylem, which extends throughout the whole interior of the stele. The primary tracheides, with the exception of those adjoining the protoxylem, have numerous bordered pits.

4. *The Secondary Tissues*.—The secondary wood, when present, has essentially the same structure as in *Lyginodendron*. Cambium and phloëm, in the normal position, are fairly preserved in some of the specimens.

5. *The Cortex*.—The most characteristic feature here consists in the horizontal plates of sclerotic cells in the inner cortex. Their structure is precisely that of the "stone-cells," found in the cortical tissues of many recent plants. Their presence in the cortex of the stem is a point of difference from *Lyginodendron*, where the sclerotic masses are usually limited to the pith and pericycle.

6. *Branching of the Stem*.—In one specimen a young stem bears a branch much smaller than itself. This is the only distinct case of branching observed in either genus. All other supposed branches have turned out to be either petioles or adventitious roots.

## B. The Leaf.

1. *Connexion between Leaf and Stem*.—The bases of petioles, in connexion with the stem, have been observed both in transverse and longitudinal section. These specimens show that the cortical tissues of the petiole have the same characteristic structure as those of the stem; we are thus enabled to recognise the petioles of *Heterangium Griecii* when detached from the stem. Unlike *Lyginodendron*, the

petiole of *H. Grievii* usually receives from the stem a single bundle only.

2. *Form and Structure of the Leaf*.—We find innumerable portions of petioles, varying from 4 mm. to 0.4 mm. in diameter, sometimes branching, and intermixed with fragments of leaflets. We can only infer that the leaf of *H. Grievii* was a highly compound one, probably not very different from that of *Lyginodendron*.

The petiole is traversed by a single bundle of *concentric* structure. Thus the bundles underwent the same change on entering the leaf as in *Lyginodendron*.

The petioles bear a considerable resemblance to those of the latter plant, from which they chiefly differ in the absence of cortical outgrowths.

### C. The Root.

1. *Connexion between Root and Stem*.—In several cases endogenous appendages, evidently adventitious roots, have been found arising from the stems of *H. Grievii*. In one specimen the bases of three such roots are seen in a vertical row, and the connexion of their tissues with those of the stem can be exactly traced.

2. *Structure of the Root*.—We have good evidence, though not so direct as in the case of *Lyginodendron*, that the roots of *H. Grievii* also belonged to the "*Kaloxylon*" type. A special form of root, with a large tetrarch stele of characteristic shape, seems to be peculiar to *Heterangium*.

### D. Habit and Dimensions of the Plant.

In habit, *Heterangium Grievii* must have been similar to *Lyginodendron*, but its dimensions were considerably smaller. Sporangia, like those of ferns, have occasionally been found in close association with the foliage, but not in connexion with it.

#### ii. *Heterangium tiliceoides*, Will.

This species differs from *H. Grievii* in several points, but evidently belongs to the same genus. The specimens are from the coal-measures of Halifax,\* and are remarkable for the astonishing perfection with which the histological structure is preserved. The general anatomy was fully described in 1887. In primary structure the stele agrees with that of *H. Grievii*, but the peripheral bundles are more distinct. The secondary tissues are subdivided by broad primary rays (enormously dilated in the phloëm), which correspond to the conjunctive tissue separating the primary bundles. Cambium

\* Williamson, "Organisation," Part XIII, 1887.

and phloëm are perfectly preserved; the latter is of great thickness, almost equal to that of the secondary wood. So perfect is the preservation, that stages in the development of the tracheides from the cambium have been observed, while the compound sieve-plates on the radial walls of the sieve-tubes are quite clear. Sclerotic groups occur in the pericycle, as well as in the cortex, and the leaf-trace bundles are in pairs—two points in which this species resembles *Lyginodendron* and differs from *H. Grievii*. Thus the close relationship of the two genera, in spite of the different arrangement of the primary wood, comes out even more clearly in this species than in *H. Grievii*.

We have a single specimen of a *Heterangium*, which differs in some respects from the two species above described, and may turn out to represent a third type.

### III. Affinities of *Lyginodendron* and *Heterangium*.

The vegetative organs of these genera show a remarkable combination of fern-like and cycadean characters. The leaves of *Lyginodendron*, which are now well known, are so like fern-leaves, not only in form and venation but in minute structure, that if they stood alone they would, without hesitation, be referred to Filices. Although many leaves simulate those of ferns in external characters (*Stangeria*, *Thalictrum*, &c.), none are known which at the same time show the characteristic anatomy of fern-leaves. Hence we are led to attach great weight to the characters of the *Lyginodendron* foliage. That of *Heterangium*, though less well preserved, was evidently of the same type.

In *Heterangium* the primary structure of the stem is much like that of a monostelic fern such as *Gleichenia*, but the leaf-trace bundles closely resemble the foliar bundles of a Cycad.

In *Lyginodendron* the whole structure of the stem suggests a Cycad, but with the remarkable peculiarity that the bundles here have the structure which in Cycadæ is usually (though not always) limited to those of the leaf. The cycadean characters are too marked to be accidental, though the general anatomy of *Lyginodendron* is not inconsistent with a close relationship to ferns, for in *Osmunda* we have a monostelic fern, with a large pith, collateral bundles in the stem, and concentric ones in the leaf.\* The mere occurrence of secondary growth in a fern-like plant is not surprising, considering that it takes place in *Botrychium* and *Helminthostachys* at the present day.

In various respects *Lyginodendron* and *Heterangium* have points in common with Gleicheniaceæ, Osmundaceæ, Marattiaceæ, Ophioglossæ,

\* See Zenetti, 'Botanische Zeitung,' 1895.

and Cycadæ. The view of their affinities, which we suggest, is that they are derivatives of an ancient generalised race of ferns, from which they have already diverged considerably in the cycadean direction. Of the two genera, *Heterangium* appears to be geologically the more ancient, and certainly stands nearer to the filicinean stock. *Lyginodendron*, while retaining conspicuous fern-like characters, has advanced much further on cycadean lines. This view by no means involves the improbable assumption that these plants were the actual ancestors of existing Cycadæ. How far their divergence from the fern stock had proceeded cannot be determined until we are acquainted with their organs of reproduction.

The existence of a fossil group on the border-land of ferns and Cycads seems now to be well established. Count Solms-Laubach places his *Protopitys* in this position, which is probably shared by *Myeloxylon* and *Poroxylon*. Messrs. Bertrand and Renault have indeed endeavoured to derive the last-named genus from *Lycopodiaceæ*, and have extended the same view to *Lyginodendron* and *Heterangium*. In the latter cases their theory is completely negatived by the organisation of the leaves, and by many structural details.

The relation of the genera which we have described to those ancient gymnosperms, the *Cordaiteæ*, will form one of the most interesting palæobotanical problems of the future.

The paper is illustrated by micro-photographs and by camera-lucida drawings.

#### IV. "On the Origin of the 'Triradiate Spicules of *Leucosolenia*."

By E. A. MINCHIN. Communicated by Professor LANKESTER, F.R.S. Received April 30, 1895.

(Abstract.)

In *Leucosolenia coriacea* the youngest spicules are found to be surrounded by six cells, which are similar in all their characters to the cells of the external flat epithelium of the sponge, and undoubtedly derived from this layer. It appears that three cells of the external epithelium wander inwards, and give rise to six by division of each cell into two, the six cells being arranged in such a way, that three are placed more internally, *i.e.*, towards the gastral surface of the body wall, and three more externally, towards the dermal surface. Each of these sets of three cells has a form which might be compared to a trefoil, and the whole mass may be described as two such trefoils superposed, the cells of one trefoil exactly corresponding to those of the other.

The spicule is formed by the three inner cells, a ray being formed

by each cell. In many instances it appears as if the three rays were formed quite separately and afterwards fused at the centre.

The three outer cells soon lose their rounded form, and by throwing out processes, assume an amoeboid appearance. After the spicule rays have attained a length of 10 or 15  $\mu$ , the three outer cells are no more to be found, having apparently rejoined the flat epithelium from whence they came. The three inner cells alone secrete the rays and continue to do so until the spicule is full grown.

The spicule rays soon appear to project beyond their formative cells, but are in reality covered by a thin layer of protoplasm. At the same time, the spicule sheath makes its appearance as a denser layer of substance between the protoplasm of the formative cell and the calcareous spicule, and it is by continued calcification of the sheath that the spicule grows.

The spicule rays attain their full thickness at their bases before they have reached their full length. The formative cells remain at the bases of the rays until this portion is built up to its full thickness. Each formative cell then migrates along its ray towards the tip, building up the ray to its full thickness as it goes. In the fully formed ray the formative cell is found adherent to the extreme tip.

#### *Theoretical considerations.*

(1.) The origin of the spicule-forming cells—that is to say, of the whole connective tissue system in these sponges—from the external flat epithelium, is another nail in the coffin of the so-called mesoderm in these forms. Sponges are to be regarded as two-layered animals, composed of a dermal and a gastral layer. The dermal layer is differentiated into (1) an external flat contractile epithelium, the neuro-muscular system, and (2) an internal connective tissue layer. The gastral layer consists of the collar cells. The amoeboid wandering cells are perhaps also to be reckoned with the gastral layer.

(2.) The fact that each ray of a triradiate spicule is formed by a single cell, shows that each triradiate spicule must be regarded as derived from the fusion of three originally separate monactinal spicules. This supports Schulze's theory, namely, that the triradiates of the more primitive Ascons have arisen as an adaptation to the structure of the sponge, and goes against Dreyer's theory that the primitive spicule of all sponges is a tetraxon, a form explained by him as the direct mechanical outcome of the vesicular structure of living bodies.

V. "Experimental Degenerations following Unilateral Lesions of the Cortex Cerebri in the Bonnet Monkey (*Macacus Sinicus*). By E. LINDON MELLUS, M.D. Communicated by Professor V. HORSLEY, F.R.S. Received May 1, 1895.

(From the Pathological Laboratory of University College, London.)

(Abstract.)

The object of this investigation was to trace by the so-called anatomical method the degeneration resulting from minute lesions of the motor area of the cortex cerebri through the brain and spinal cord, to locate the path of the conducting fibres in the internal capsule and elsewhere, to follow them as far as possible to their destinations, and by such control observations to check off the results obtained by previous excitation experiments.

*Method Pursued.*

The animal selected for these experiments was the bonnet monkey (*Macacus Sinicus*), and the lesions were made in the motor area of the left hemisphere. The records are here presented of degenerations resulting from fourteen (14) successful operations, three being lesions of the hallux centre, four of the thumb centre, and seven of four separate centres in the facial area. The animal being etherised the skull was opened under strict aseptic precautions, and the centre to be removed located by stimulation with a weak faradic current. A small portion of the cortex (generally about 16 sq. mm.) embracing this centre was then excised, care being taken to remove with it a portion of the underlying corona radiata, thus ensuring the removal of all the cortical cells. The scalp wound was closed with horse-hair sutures and covered with borated cotton held in position by collodion. The wound in every case healed by first intention and but slight and transient paresis resulted. The animals were killed in from 10 to 35 days after the operation, the brains and cords hardened in Müller's fluid and stained by the Marchi method.

*Degeneration following the Hallux Lesions.*

The portion of the cortex removed in these cases was taken as nearly as possible from the centre of the triangular space at the upper extremity of the ascending frontal convolution formed by the longitudinal fissure and the fissure of Rolando, and extending down the convolution to the posterior extremity of the superior frontal sulcus.

Numerous degenerated association fibres, both coarse and fine, were found passing to the central convolutions down to the level of the

inferior genu of the fissure of Rolando; fine fibres only passed to the superior parietal lobule; degenerate fibres, mostly fine, were traced to the posterior portion of the superior frontal convolution; both coarse and fine to the lobulus paracentralis; fine only to the precuneus, and a very little fine degeneration to the gyrus fornicatus. The degenerate fibres crossing in the corpus callosum were very fine and occupied the middle third of its antero-posterior extent. This corresponded very closely with the antero-posterior extent of the ascending parietal convolution at this level. Degenerate fibres could be traced into the convolutions of the right hemisphere corresponding to the distribution of the degeneration in the left, but considerably less in amount, and the area of distribution being rather less extensive. Both coarse and fine degenerate fibres, varying considerably in amount in different animals, pass from the lesion through the mesial half of the centrum semiovale to the left internal capsule, in the lower levels of which they are located in the middle third of the posterior limb. From the internal capsule most of the fine degeneration passes into the optic thalamus. In the left crus the degeneration is very evenly scattered over the middle third, and many coarse degenerate fibres pass from here into the substantia nigra. At the decussation of the pyramids the tract divides, the larger portion crossing to the opposite lateral column while the smaller goes to that of the same side (*vide Preliminary Report\**). The amount of degeneration passing to the lateral column of the same (left) side varies from a third of all the degeneration in one case to about a twentieth in the other two. In each case a small number of degenerate fibres remain in the left anterior column after the completion of the decussation. The amount varies in different cases and is not apparently dependent on the proportion of degenerate fibres passing to the lateral column of the same side. The relations and extent of the degenerated areas remain unchanged throughout the cervical and dorsal cord. The degeneration in the crossed tract of each side is evenly scattered over its entire area, the two sides only differing in the density of the degeneration.

In the upper cervical region the tendency of the crossed tract to mingle with or encroach on the boundaries of the direct cerebellar tract is well illustrated. In the lumbar region the degeneration in each crossed tract and in the left anterior column (Tooth) begins to go out, and in the only case examined at that level the degeneration had not all disappeared at the level of the third sacral root.

*Degeneration following Lesions of the Thumb Centre.*

The portion of cortex removed in these cases was from the ascending parietal convolution between the lower extremity of the

\* 'Roy. Soc. Proc.,' 1894.

intra-parietal sulcus and the fissure of Rolando, and a little above the inferior genu of the fissure of Rolando (Beever and Horsley). In these cases the majority of the degenerated association fibres pass to the ascending frontal and ascending parietal convolutions, rather more to the latter than to the former. In these lesions of the thumb centre degenerate fibres were distributed to these two convolutions from the border of the longitudinal fissure nearly to the fissure of Sylvius, the fibres passing upward from the lesion being coarse, while those passing downward were fine. Fine degeneration was traced into the posterior portion of the middle and inferior frontal convolutions, and in one case to the supra marginal and angular gyri. In two cases it was also found passing to the upper or posterior portion of the superior temporal convolution. In various instances a few degenerate fibres passed to the precuneus and lobus quadratus and paracentralis and to the gyrus fornicatus.

There is considerable variation in the size of the fibres crossing in the corpus callosum after lesion of the thumb centre. With slight variations it was situated in the middle third. The distribution of the degenerate fibres to the convolutions of the right hemisphere corresponds very closely to that in the left, though less in amount and slightly less in area. The same arrangement of the coarse and fine fibres observed on the left side prevails on the right—coarse above the lesion, fine below. The degeneration passing from the lesion downward through the centrum semiovale is both coarse and fine. These fibres are more scattered and the area of degeneration is greater than that in lesions of the hallux centre, both laterally and antero-posteriorly. In one case only was there any apparent separation of the coarse and fine fibres in the centrum semiovale, and there the coarser fibres were anterior to the fine. The same arrangement prevails in the internal capsule, the coarser fibres representing the pyramidal fibres being found (in the lower levels) in the middle third of the posterior limb, while the fine fibres, mostly representing the corona radiata thalami, are largely situated in the posterior third of the posterior limb. Between the upper and lower levels of the capsule there is a movement of the degeneration forward, and in the lower levels most of the fine degeneration has already passed into the thalamus. In one case a little fine degeneration was found in the right internal capsule occupying a position corresponding to that of the fine degeneration in the left capsule. This all passed into the right thalamus. In another case both coarse and fine degenerate fibres were found in the right capsule occupying the same position and pursuing the same course as the corresponding fibres on the left side. These fibres come from the area of degeneration in the right centrum semiovale, into which the fibres crossing in the corpus callosum could be traced, but no continuous fibres could

be traced from the corpus callosum into the right internal capsule. The coarse degeneration, mixed with more or less fine, is found in the middle third of the crus. In each case some fine degeneration reaches the crus, where it invariably takes its position external to the coarse fibres. In the entire group a large proportion of the degeneration passes to the substantia nigra. This varies from a half to nearly the whole of the degeneration reaching the crus. The fine degeneration referred to external to the coarse disappears from the crus, but could not be traced to its destination. The coarse degeneration observed in one case in the right internal capsule reached the crus, where all but a few fibres disappeared. In one case the last remaining degenerate fibres disappeared in the upper levels of the pons. In another about half those remaining disappeared in the lower pons and upper medulla, but could not be traced after leaving the pyramidal tract. In one case only was there a division of the degenerated tract at the decussation of the pyramids, such as was observed in lesions of the hallux centre, and the amount of degeneration passing to the left lateral column was less than in either of the hallux cases. This was also the only case in which a few degenerate fibres remained in the left anterior column after the completion of the decussation. In one of the remaining cases the few degenerate fibres remaining in the left anterior column cross to the right lateral column, and in the other the degeneration in each pyramid apparently goes to the crossed tract of the opposite side. In each of the three cases in which the degeneration reaches the cervical cord, its amount and relations remain unchanged in the upper and middle cervical regions. From the level of the seventh cervical root downward the degenerate fibres steadily and gradually disappear, and at the level of the third dorsal root there are none left, thus confirming the results obtained by excitation of the nerve roots (Ferrier and Yeo, Forgue, &c.).

*Degeneration following Lesions of the Facial Area.*

Seven successful experiments were performed to determine if possible the degeneration resulting from minute lesions within the facial area. In four of these the lesion was practically the same, the portion of cortex removed being just above the fissure of Sylvius and just anterior to the plane of the fissure of Rolando, representing the movement of opening the mouth straight; in one the lesion was in the same level, and just posterior to the fissure of Rolando, and the movement elicited on stimulation was pursing the mouth towards the opposite side; in one the lesion was just above the fissure of Sylvius and anterior to the plane of the sulcus transversalis frontalis inferior, and the movement represented was the

rhythmical movement of mastication. In the other experiment the portion of cortex removed was in the level of the inferior genu of the fissure of Rolando, between the fissure of Rolando and the precentral sulcus, and the movement obtained on stimulation was elevation of the opposite angle of the mouth. None of the brains in this group were examined above the level of the inferior genu of the fissure of Rolando. In three cases, one of lesion of the mastication centre and two of lesion of the centre for opening the mouth, only fine degeneration resulted from the lesion; in the other four both coarse and fine. In two cases of "opening mouth" and in the lesion posterior to the plane of the fissure of Rolando, coarse degenerate association fibres were only found in the upper levels of the lesion or above, where they were mixed with more or less fine, the proportion of coarse to fine increasing from the lesion upward, while the aggregate amount of degeneration decreased. In all the lesions on the border of the fissure of Sylvius anterior to the fissure of Rolando, most of the degeneration was found in the ascending frontal convolution from the border of the fissure of Sylvius to the level of the inferior genu of the fissure of Rolando. In the lesion of the mastication centre all the degeneration was very fine, and in the left hemisphere, with the exception of a few fibres near the base in the ascending parietal, the degenerate association fibres were confined to the ascending frontal convolution. In the four lesions of the centre for opening the mouth degenerate association fibres were also distributed to the ascending parietal, the posterior half of the inferior frontal, and the posterior or upper extremity of the superior temporal convolutions. In only one of these four cases was any degeneration found in the inferior temporal convolution. In the lesion posterior to the plane of the fissure of Rolando both coarse and fine degeneration was distributed to the central convolutions, the fine degeneration above the lesion being mixed (as noted above) with coarse fibres, which were more numerous in the ascending parietal convolution. No degeneration was traced into the frontal convolutions in this case, but a good deal of coarse degeneration passed from the superior temporal convolution to the posterior extremity of the internal capsule, where it turned downward and forward in the posterior limb. A mingling of this with the degeneration in the internal capsule coming from the lesion could not be demonstrated. In the case of the lesion on the upper border of the facial area the coarse degenerated association fibres are almost entirely confined to the neighbourhood of the lesion. In addition to this, fine degeneration is distributed to the central convolutions nearly to the fissure of Sylvius. The degeneration in the supra-marginal gyrus and in the inferior frontal convolution is much more than in any other experiment, the degeneration in the latter convolution reaching to the anterior extremity of the precentral

sulcus. No degeneration in this case was found in the temporal lobe. In all directions the amount of degeneration decreased as the distance from the lesion increased.

The size of the degenerate fibres crossing in the corpus callosum corresponded to that of the fibres proceeding from the lesion, *i.e.*, in each case in which both coarse and fine fibres were found proceeding from the lesion both coarse and fine fibres crossed in the corpus callosum. The coarse fibres in the corpus callosum were finer than the coarse fibres elsewhere, and much less numerous than the fine fibres. In all the lesions anterior to the plane of the fissure of Rolando the degeneration was in the posterior two-thirds of the anterior half of the corpus callosum. Following the one lesion posterior to the plane of the fissure of Rolando it was in the middle third, thus corresponding to the hallux and thumb lesions. The convolutions of the right hemisphere were examined in four cases, and, with the exception of the case of the lesion of the mastication centre, the degeneration, while corresponding to that on the left side, was rather less extensive and less in amount. The majority of the degeneration was in all cases distributed to the central convolutions. In the case of the lesion of the mastication centre the degeneration was even more extensive and of about equal amount on the right side. In most of these cases but little of the degeneration passing from the lesion to the left internal capsule appears in the centrum semiovale as it reaches the capsule by passing over and around the anterior angle of the putamen at a lower level. As much of this as reaches the centrum semiovale, together with the fibres going to the corpus callosum, occupies a more extensive area than in either the hallux or thumb lesions, corresponding to the antero-posterior extent of both central convolutions. The angle of the mouth centre was the only case in which both coarse and fine fibres degenerated from the lesion in which there was not marked grouping of the coarse fibres anterior to the fine.

In all the lesions of the facial area the degenerations in the uppermost levels of the capsule, *i.e.*, the coarser fibres which pass down *through* the capsule, are situated in its anterior portion, and in the lower levels move backward till they occupy in the lower levels the middle third of the posterior limb. Between the upper and lower levels of the capsule most of the fine degeneration passes from the posterior limb into the thalamus. In the lesion of the mastication centre, though the degeneration in the capsule is all fine, but little passes to the thalamus. In the three cases referred to in which there were only fine degenerate fibres, a varying amount of fine degeneration was observed in the posterior limb of the right internal capsule corresponding in position to that in the left. In one opening of the mouth lesion the amount on the right side was nearly

half as much as on the left, in another it was nearly equal on the two sides, and in the lesion of the mastication centre there was quite as much on the right as on the left side. In the other four lesions of the facial area there was no degeneration observed in the right internal capsule. In the lesion of the centre for the angle of the mouth degenerate fibres were observed in the striations of the right thalamus, some of them being pretty coarse. In two cases fine degeneration was observed crossing in the posterior commissure. In the three cases in which fine degeneration only resulted from the lesion, most of this disappears before reaching the crus. In the lesion of the mastication centre it could not be traced, but in the other two it passed to the thalamus from both left and right capsule. In all three a small amount of fine degeneration was found in the middle third of both crura. In one case this passed to the substantia nigra, but in the other two it could not be traced. In the other four lesions of the facial area the coarse degeneration from the left internal capsule was scattered very evenly over the middle third of the crus encroaching a little upon the lateral third. Some of the fibres passed into the substantia nigra or the sub-thalamic region, and in one case a considerable number to the anterior corpus quadrigeminum. The remaining degenerate fibres begin to leave the left pyramid at the junction of the pons and medulla, passing as single degenerate fibres to the facial nucleus of one or the other side. Below the level of the facial nuclei these fibres pass to the motor nuclei of the glosso-pharyngeus and the vagus on both sides, the majority crossing the raphe to reach the nucleus on the opposite side. Occasional fibres were observed, which apparently passed to some termination dorsal to these nuclei. This movement of degenerate fibres continued as far as the sensory decussation. A few degenerate fibres (probably thumb or finger fibres) remained in the pyramid, and crossed in the decussation to the right lateral column, and disappeared in the lower cervical or upper dorsal region. No degenerate fibres remained in the left anterior column after the decussation, and there was no apparent division of the tract, as in hallux and thumb lesions.

Under the head of "other appearances resembling degeneration" are recorded appearances which are of doubtful significance, but yet so closely resemble degeneration that they are well calculated to deceive and lead one into grave error. Among these may be mentioned apparent degeneration in the left lateral geniculate body and optic radiation, in the posterior longitudinal fasciculi, the roots of all the motor cranial nerves of both sides and the corpus trapezoides (Marchi), and in one case extensive degeneration in the optic tract. The degenerations in the roots of the motor cranial nerves and in the corpus trapezoides could hardly have been due to the lesions, as

exactly the same appearances were found in the brains of control animals in which there was no lesion.

In the general summary attention is called to the fact that the distribution of the degenerate association fibres in the thumb lesions corresponded with the measurements made by Bevan Lewis of the corpuscles of the fourth layer of the cortex in this region, *i.e.*, that coarse fibres were distributed to the upper part of the motor area and fine fibres to the lower part. In four of the experiments (one thumb and three facial) neither pyramidal fibres nor fibres connecting the nuclei of the cranial nerves directly with the cortex were found to have degenerated. Yet in each of these cases the characteristic movement was obtained from stimulation of the area removed quite as readily as in any other case. The degenerate fibres passing through the internal capsule in these cases apparently belong to two distinct systems, which are also more or less represented in the other experiments. One of these, composed almost entirely of fine fibres, passes from the posterior limb of the internal capsule into the outer surface of the optic thalamus, and represents the corona radiata thalami. These fibres are to some extent mixed with the pyramidal fibres in the posterior limb of the internal capsule, but are mostly situated in the posterior third, and, as they pass from there into the thalamus, make room for the entrance of the sensory (non-excitabile) tract. The other of the two systems referred to, largely composed of coarser fibres, passes through the internal capsule into the crus, and apparently ends in the substantia nigra. These fibres are of much the same calibre, and apparently occupy the same position in the internal capsule and crus as the true pyramidal fibres. Both these tracts appear to arise in all portions of the motor cortex coming within the range of these experiments. All the degenerated pyramidal fibres from the hallux and thumb lesions were found to enter the capsule at or near the posterior extremity, while the corresponding fibres from the facial lesions entered the capsule at or near the anterior extremity, and the former were displaced forward and the latter backward until in the lower levels of the capsule they all found a place in the middle third of the posterior limb. It is also shown that a line can be drawn from the fissure of Sylvius upward, so dividing the motor area into two parts, that all the facial lesions from which fibres enter the anterior portion of the capsule would be in the anterior division, and all the hallux and thumb lesions from which fibres enter the posterior portion of the capsule would be in the posterior division. In the movement of the facial fibres backward between the upper and lower levels of the capsule they would necessarily, at some level, envelop the genu, which would account for the fact that they have always been described as occupying that position. The location of these degenerations in the internal capsule

corresponds very closely with the results of excitation of the fibres in the living animal of the same species (Beever and Horsley). In all the cases in which there was coarse degeneration in the internal capsule it was, with two exceptions (both hallux cases) grouped on the outer edge of the capsule. Attention is called to the fact that a large proportion of the coarser fibres passing down through the capsule enter the substantia nigra, and these experiments show this tract to be nearly or quite as large as that passing down into the pyramid. These are apparently fibres which have been looked upon as pyramidal, and, as the "pyramidal tract" has been shown to be even more extensive in the medulla and below the decussation than in the internal capsule, it follows that the fibres passing to the substantia nigra are probably replaced by others arising at lower levels. These degenerations show that in the monkey the facial fibres are situated in the middle third of the crus, in which they are mingled with the fibres of the pyramid, and that they do not occupy a space by themselves mesial to the pyramid.

VI. "On the Cause of the Differences in Lichtenberg's Dust-Figures: Preliminary Note." By SILVANUS P. THOMPSON, D.Sc., F.R.S. Received May 9, 1895.

As ordinarily produced by dusting a mixture of red-lead and lycopodium upon a surface which has been charged by contact with the knob of a Leyden jar, the dust-figures present a remarkable and hitherto unexplained difference of form. The positive figures consist of white lines branching in stellate or dendritic patterns, whilst the negative figures exhibit red patches of circular or ovate outline. The differences, save in the matter of colour, are not due to the powders used nor to the nature of the dielectric surface chosen for the experiment. They vary only slightly with the nature of the gas; but are more considerably altered by the rarefaction of the air. The author found that the dendritic patterns of the positive figures are correlated to the brush form of discharge, whilst the rounded patches of the negative figures are due to the silent discharge of electrified winds. When polished metal surfaces are used in air for producing the discharges (as in the usual case when the knob of a Leyden jar is employed), negative electrification more readily discharges itself in a wind, positive electrification less readily, disruptively, as a brush. But where a smooth surface of a peroxide, such as the peroxide of lead, is substituted for a metal knob, positive electrification will discharge itself as a wind, giving rise to white positive figures of rounded outline; while negative electrification will under certain conditions produce a brush discharge from the peroxide surface,

giving rise to red dendritic patterns. The author considers these differences to be analogous to the differences observed in the experiments of Oliver Lodge upon the photo-electric loss of charge first observed by Hertz.

VII. "Theorems on the Attraction of Ellipsoids for certain Laws of Force other than the Inverse Square." By E. J. ROUTH, F.R.S. Received May 11, 1895.

(Abstract.)

The object of the author is to find finite expressions for the potentials of an ellipsoidal shell, and of a solid ellipsoid when the law of force is the inverse  $\kappa^{\text{th}}$  power of the distance,  $\kappa$  being positive or negative. It is shown in the beginning of the paper that the two cases in which  $\kappa$  is an even integer and an odd integer require different treatment.

After discussing some special cases, we come to the first general theorem. Supposing that  $\kappa$  is even and that the shell is a thin homogeneous homœoid, the potential is found to assume very different forms according as  $\kappa$  is greater or less than 3, so that the law of the inverse square is just on one side of the boundary. When  $\kappa > 3$ , the potential can be completely integrated, and an expression is found containing  $\frac{1}{2}(\kappa-2)$  terms, and involving only the differentiation of an integral rational function of  $xyz$  of  $\kappa-4$  dimensions. The general form at an internal point is

$$V = \frac{2\pi\mu}{(\kappa-1)(\kappa-3)} \left( \frac{2}{E} \right)^{\kappa-3} \left\{ 1 + \frac{1}{2^2} \frac{E\Delta}{\kappa-4} + \frac{1}{2^4} \frac{E^2\Delta^2}{1.2(\kappa-4)(\kappa-5)} + \dots \right\} P,$$

where

$$P = (a^2x^2 + \beta^2y^2 + \gamma^2z^2)^{\frac{1}{2}(\kappa-4)}$$

$$E = 1 - ax^2 - \beta y^2 - \gamma z^2$$

$$\Delta = \frac{1}{\alpha} \frac{d^2}{dx^2} + \frac{1}{\beta} \frac{d^2}{dy^2} + \frac{1}{\gamma} \frac{d^2}{dz^2}.$$

When  $\kappa$  is  $< 3$  the potential takes the form of a single integral

$$V = \frac{2\pi\mu}{\kappa-1} \int_0^\infty u^{-1} du \left( u^2 \frac{d}{du} \right)^t \frac{abc u^{\frac{1}{2}}}{Q|t} \left( 1 - \frac{x^2}{a^2+u} - \frac{y^2}{b^2+u} - \frac{z^2}{c^2+u} \right)^t,$$

where  $t = \frac{1}{2}(2-\kappa)$ . This reduces to the ordinary well-known form when  $t = 0$ , i.e., when the law is the inverse square.

Proceeding next to a thin heterogeneous homœoid, the density being  $\phi(\xi\eta s)$  where  $\phi$  is a function of  $i$  dimensions, different cases

are found to arise according as  $\kappa$  is greater or less than 3 and  $i$  greater or less than  $\kappa-2$ . If  $\kappa > 3$  and  $i < \kappa-2$  the potential can be completely integrated. A finite expression is given containing not more than  $\kappa-3$  terms, and involving differential coefficients of an integral rational function of  $xyz$ . For an internal point the general form is

$$V_1 = \frac{2\pi\mu}{\kappa-1} \frac{1}{L(\kappa-3)} \Sigma \frac{L(h)}{L(\kappa-4-h)} \left(\frac{2}{E}\right)^{h+1} (\Delta' + \frac{1}{2}\Delta)^{\kappa-4-h} (a^2x^2 + b^2y^2 + c^2z^2)^{\frac{\kappa-4}{2}} \phi$$

where  $\Delta$  and  $\Delta'$  are two differential operators, and  $\Sigma$  implies summation from  $h=0$  to  $\kappa-4$ . The potential is also found for an external point. If  $i > \kappa-2$  or  $\kappa < 3$  the potential contains a single integral which reduces to a known form when we can put  $\kappa=2$ . There are two standard expressions, one of which is of the form

$$V_2 = \frac{2\pi\mu}{\kappa-1} \frac{|t|}{|2t|} \int_0^\infty \frac{abc \, du}{Q} M \psi \left( \frac{ax}{a^2+u}, \frac{by}{b^2+u}, \frac{cz}{c^2+u} \right),$$

where 
$$M = \frac{D^t}{|t|} + \frac{uRD^{t+1}}{|t+1|2^2} + \dots, \quad t = \frac{\kappa-2}{2}.$$

$D$  is a differential operator and  $R$  a quadratic function of  $xyz$ , which are explained in the paper.

Examples are given throughout to illustrate the mode in which these general formulæ are to be used, and full references to all other writings on the same subject as far as they are known to the author.

Passing on to a solid ellipsoid the potentials at an internal point for both a homogeneous and a heterogeneous ellipsoid are discussed both when  $\kappa >$  and  $< 3$  and finite expressions are found, which reduce to known forms when  $\kappa=2$ . When the point is external and the strata are similar ellipsoids with any law of density, expressions for the potential are found for the cases  $\kappa=4, 6, 8$ , and  $10$ . These are reduced to depend on a single integral. Except when  $\kappa=4$ , these can be completely integrated when the solid is homogeneous and in some other cases.

When  $\kappa$  is an odd integer, there is a division of cases according as  $\kappa$  is  $<$  or  $> 2$ . In the first of these cases, finite expressions for the potential of a thin homœoid are found (1) when homogeneous, and (2) when heterogeneous. There are corresponding expressions for a solid ellipsoid. As explained in the text, these results differ in form rather than in substance from some already known, but they are treated in a different manner.

In the second case, when  $\kappa > 2$ , the integrations become very long. Finite expressions for the potential of a homogeneous homœoid are, however, found (1) when the force varies as the inverse cube, and (2)

when the force varies as the inverse  $\kappa^{\text{th}}$  power, and  $\kappa > 3$ , the formulæ for the inverse cube being much simpler than for any greater power.

Lastly, the potential of a special elliptic disc is discussed which has the property that the level surfaces are confocals, the force varying as any odd inverse power greater than the square.

In this list only those properties have been mentioned which the author believes to be new.

*Presents, June 13, 1895.*

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June 20, 1895.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

Mr. J. Wolfe Barry, Professor A. G. Bourne, Professor J. R. Green, Mr. E. H. Griffiths, Mr. C. T. Heycock, Professor S. J. Hickson, Major Holden, Prof. W. MacEwen, Dr. S. Martin, Professor G. M. Minchin, and Mr. W. H. Power were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On the Magnetical Results of the Voyage of H.M.S. 'Penguin,' 1890-93." By E. W. CREAK, Captain R.N., F.R.S. Received May 8, 1895.

(Abstract.)

In view of the numerous magnetic surveys of different countries which have been made in recent years, it seems important to call attention to the increasing value of magnetic surveys made over that much larger area of the earth, the sea; the coasts washed by the sea; magnetic disturbances proceeding from land under the sea, and the settlement of the question of the direction of the iso-magnetics when passing from deep water to the land.

The magnitude of the subject is self evident, therefore it is presumed that under existing circumstances of paucity of information, any well conducted series of observations of the kind required will be welcomed.

H.M. surveying ship "Penguin" being ordered to survey the West Coast of Australia, proceeding there by way of the Suez Canal, it was decided to equip that vessel with the necessary absolute magnetic instruments for observations on land and other necessary instruments for relative observations on board the ship. The "Penguin" being practically an iron ship, general observations of the magnetic elements at sea were not undertaken, but the special attention of the observing officers was directed to obtaining the following observations:—

- (1.) The magnetic elements on land at all ports visited.
- (2.) Local magnetic disturbances on islands.
- (3.) The magnetic survey of the West Coast of Australia, which, until the "Meda's" observations in 1885-86, was a *terra incognita* as far as the inclination and force were concerned.
- (4.) The position and extent of an area of remarkable local magnetic disturbance, reported by H.M. surveying vessel "Meda" as situated under the sea at Port Walcott (N.W. Australia) and two miles from the visible land.

With regard to (1)—A series of observations with the absolute instruments were made at twenty-three different places situated on the route from Malta, *via* Australia, to Hong Kong. These are shown in Table I.

For (2)—Observations of local magnetic disturbance on land were made at Perim Island and Baudin Island (N. Australia). At Perim the principal disturbance was found in the inclination, being about  $1\frac{1}{2}^{\circ}$  to  $2^{\circ}$  in value. At Baudin Island, the declination was disturbed  $5\frac{1}{2}^{\circ}$ , the inclination  $2\frac{3}{4}^{\circ}$ . Horizontal force undisturbed.

For (3)—The magnetic elements were observed at twelve stations between King George's Sound and Baudin Island.

With regard to (4), the chief interest lies in the area of disturbance termed a "magnetic shoal," from its situation at 9 fathoms below the sea level. Four days were devoted to the survey of this shoal, whilst observations were being made on the neighbouring land. (Data contained in Tables III and IV.) This survey was necessarily carried out on board the ship with the relative instruments.

The results were the mapping of an area of magnetical disturbance  $3\frac{1}{2}$  miles long by  $1\frac{3}{4}$  miles average width, extending in a north-easterly direction. The positions of the greatest easterly disturbance and westerly disturbance having been found in proximity to one another, the ship was moored in the neighbourhood, and observations of declination, inclination, and total force made. From these a sectional

plan of the magnetic rock causing the disturbance has been made, and, together with the complete set of observations, the following results have been deduced.

The cause of the disturbance is a ridge of rock permanently magnetized, repelling the north-seeking end of the needle. In the transverse section this ridge is steeper on the south-east side where the disturbances are at a maximum than on the north-west side. Longitudinally the ridge rises rather abruptly to a principal peak (as determined by the point of maximum disturbance of the vertical force), followed by a depression, and a second peak, finally falling abruptly to the level.

The principal values of the disturbances caused by this ridge are—

|                    |                                           |
|--------------------|-------------------------------------------|
| Declination.....   | 56° E. on S.E. side, 26° W. on N.W. side. |
| Inclination.....   | −29°.                                     |
| Horizontal force.. | −1·92 on S.E. side; +1·04 on N.W. side.   |
| Vertical force...  | −4·44 metric units.                       |

A geological survey of the coast at the Red Cliff (see map), where the greatest disturbances of the magnetic elements were observed, was made, and specimens of rock and sand were obtained which have since been tested for susceptibility. The evidence from these does not give any direct information tending to show the exact nature of the rock causing the remarkable disturbances over the magnetic shoal, but the character of the disturbances caused by the visible and invisible land are of a similar character.

Five diagrams are appended showing the data from which the foregoing results have been deduced, with a map showing the position of the “magnetic shoal” relative to the neighbouring land. Also a geological map of the Red Cliff and neighbourhood.

- II. “A Dynamical Theory of the Electric and Luminiferous Medium. Part II; Theory of Electrons.” By JOSEPH LARMOR, F.R.S., Fellow of St. John’s College, Cambridge. Received May 16, 1895.

(Abstract.)

In a previous paper on this subject,\* it has been shown that by means of a rotationally elastic æther, which otherwise behaves as a perfect fluid, a concrete realization of MacCullagh’s optical theory can be obtained, and that the same medium affords a complete representation of electromotive phenomena in the theory of electricity. The ponderomotive electric forcives were, on the other hand, deduced

\* ‘Roy. Soc. Proc.’ November, 1893; ‘Phil. Trans.’ 1894, A, pp. 719—822.

from the principle of energy, as the work of the surplus energy in the field, the motions of the bodies in the field being thus supposed slow compared with radiation. It was seen that in order to obtain the correct sign for the electrodynamic forcives between current systems, we are precluded from taking a current to be simply a vortex ring in the fluid æther; but that this difficulty is removed by taking a current to be produced by the convection of electrons or elementary electric charges through the free æther, thus making the current effectively a vortex of a type whose strength can be altered by induction from neighbouring currents. An electron occurs naturally in the theory as a centre or nucleus of rotational strain, which can have a permanent existence in the rotationally elastic æther, in the same sense as a vortex ring can have a permanent existence in the ordinary perfect fluid of theoretical hydrodynamics.

In the present paper a further development of the theory of electrons is made. As a preliminary, the consequences, as regards ponderomotive forces, of treating an element of current  $\delta s$  as a separate dynamical entity, which were indicated in the previous paper, are here more fully considered. It is maintained that a hypothesis of this kind would lead to an internal stress in a conductor carrying a current, in addition to the forcive of Ampère which acts on each element of the conductor at right angles to its length. Though this stress is self-equilibrating as regards the conductor as a whole, yet when the conductor is a liquid, such as mercury, it will involve a change of fluid pressure which ought to be of the same order of magnitude as the amperian forcive, and therefore capable of detection whenever the latter is easily observed. Experiments made by Professors FitzGerald and Lodge on this subject have yielded purely negative results, so that there is ground for the conclusion that the ordinary current-element  $\delta s$  cannot be legitimately employed in framing a dynamical theory.

This result is entirely confirmed when we work out the properties of the field of currents, considered as produced by the convection of electrons. There can be no doubt that a single electron may be correctly taken as an independent element of the medium for dynamical purposes; so that electrodynamical relations deduced from a statistical theory of moving electrons will rest on a much surer basis than those derived from the use of a hypothetical current-element of the ordinary kind, in cases where they are in discrepancy.

Now it is shown that an intrinsic singularity in the æther, of the form of an electron  $e$ , moving with velocity  $(\dot{x}, \dot{y}, \dot{z})$  relative to the quiescent mass of æther, is subject to a force  $e(P, Q, R)$ , given by equations of the form

$$P = c\dot{y} - b\dot{z} - dF/dt - d\psi/dx;$$

in which  $(a, b, c)$  is the velocity of flow of the æther where the electron is situated, and is equal to the curl of  $(F, G, H)$  in such way that the latter is Maxwell's vector potential given by the formulæ of the type

$$F = \int \frac{u}{r} d\tau + \left[ \left( B \frac{d}{dz} - C \frac{d}{dy} \right) \frac{1}{r} \right] d\tau;$$

and where  $\Psi$  is the electrostatic potential due to the electrons in the field, so that  $\Psi = c^2 \Sigma e/r$ , where  $c$  is the velocity of radiation. These equations are proved to hold good, not merely if the motions of the electrons are slow compared with radiation, as in the previous paper, but quite irrespective of how nearly they approach that limiting value; thus the phenomena of radiation itself are included in the analysis.

An element of volume of an unelectrified material medium contains as many positive electrons as negative. This force  $(P, Q, R)$  tends to produce electric separation in the element by moving them in opposite directions, leading to an electric current in the case of a conductor whose electrons are in part free, and to electric polarization in the case of a dielectric whose electrons are paired into polar molecules. In the former case, the rate at which this force works on a current of electrons  $(u', v', w')$ , is  $Pu' + Qv' + Rw'$ ; it therefore is identical with the electric force as ordinarily defined in the elementary theory of steady currents. In the case of a dielectric it represents the ordinary electric force producing polarization. So long as a current is prevented from flowing, the ponderomotive force acting on the element of volume of the medium is the one of electrostatic origin due to such polarization as the element may possess, for as the element is unelectrified it contains as many positive electrons as negative. But if a current is flowing, the first two terms of  $(P, Q, R)$ , instead of cancelling for the positive and negative electrons, become additive, as change of sign of the electron is accompanied by change of sign of its velocity; so that there is an electrodynamic force on the element of volume,

$$(X, Y, Z) = (v'c - w'b, w'a - u'c, u'b - v'a),$$

where, however,  $(u', v', w')$  is the *true* current composed of moving electrons, not the total circuital current  $(u, v, w)$  of Maxwell, which includes the rotational displacement of the free æther in addition to the drift of the electrons.

The electric force  $(P, Q, R)$  as thus deduced agrees with the form obtained originally by Maxwell\* from the direct consideration of his concrete model of the electric field, with idle wheels to

\* Maxwell, "On Physical Lines of Force," 'Phil. Mag.' 1861-63; 'Collected Papers,' vol. 1, pp. 450-512.

represent electrification. It has been pointed out by von Helmholtz and others, that the abstract dynamical analysis given in his *Treatise* does not really lead to these equations when all the terms are retained; this later analysis proceeds, in fact, by the use of current-elements, which form an imperfect representation, in that they give no account of the genesis of the current by electric separation in the element of volume of the conductor.

The ponderomotive force ( $X, Y, Z$ ) is at right angles to the direction of the true current, and is precisely that of Ampère in the ordinary cases where the difference between the true current and the total current is inappreciable. It differs from Maxwell's result in involving true current instead of total current; that is, the force tends to move an element of a material body, but there is no such force tending to move an element of the free æther itself. In this respect it differs also from the hypothesis underlying von Helmholtz's recent treatment of the relations of moving matter to æther.

When we treat of a single electron,  $(a, b, c)$  is the flow of the æther where it is situated. When we treat of an element of volume with its contained electrons,  $(a, b, c)$  becomes the smoothed out, or averaged, flow of the æther in the element of volume; it is circuital because the æther is incompressible, and thus it represents the magnetic induction of Maxwell.

When magnetic polarization of the medium contributes to the force, it is necessary to divide  $(a, b, c)$  into two parts, one part  $(\alpha, \beta, \gamma)$  contributed by the medium as a whole, and independent of the surroundings of the element, and the other representing the effect of the polarization in the immediate neighbourhood; the former part is, of course, the magnetic force of Maxwell. Similar considerations apply as regards the electric force in a polarized dielectric; it is clearly proper to define it so as to correspond to magnetic induction, not to magnetic force. It is then shown from the direct consideration of the orbital motions of electrons, that there is, in addition to the electrodynamic force on the element of volume of the material medium, a magnetic force derived from a potential function  $\frac{1}{2}\kappa(\alpha^2 + \beta^2 + \gamma^2)$ , and a force of electric origin derived from a potential  $(K-1)/8\pi c^2 \cdot (P^2 + Q^2 + R^2)$ . If the element carries an electric charge of density  $\rho$ , there is also the force  $\rho(P, Q, R)$ . In addition to these latter forces on the polarized element, there are also stresses due to interaction between neighbouring parts, in which are to be found the main explanation of the phenomena of electrostriction and magnetostriction.

As an example of these ponderomotive forces, the mechanical pressure produced by radiation is examined later on, with a result half that of Maxwell when the light is incident on an opaque body, and which gives pressures on the two sides of the interface each equal to Maxwell's

expression multiplied by  $\frac{1}{2}(1-\mu^{-2})$ , when the interface separates two transparent media.

The distinction between true current and total current is practically immaterial, except in questions relating to electrical vibrations and to optics. The remaining part of the theory is therefore developed more particularly with a view to optical applications. At the end of the previous paper a brief outline of the method of treating optical dispersion was given, and it was shown that the same principles led directly to Fresnel's formula for the effect on the velocity of light produced by motion, through the æther, of the material medium which transmits it. In the latter respect the theory is in agreement with a more recent discussion by H. A. Lorentz, of the propagation of electrical and optical effects through moving media.

A detailed theory of optical propagation in transparent and opaque ponderable media is given, on the basis that it is the contained electrons that are efficient in modifying the mode of propagation from that which obtains in free æther. The dispersive theory of MacCullagh had been physically interpreted in the earlier part of the previous paper; but it appears from the same train of reasoning as was there applied to Cauchy's theory, that molecular magnitudes are too small compared with the wave-length to allow any considerable part of the actual dispersion to be accounted for statically in that way. The rotatory dispersions, both natural and magnetic, are, however, structural phenomena; and this accounts for their smallness compared with ordinary dispersion.

As regards ordinary dispersion, a formula is obtained for the case of perfectly non-conducting media, namely,  $\mu^2 = 1 + A/(\beta^2 - p^2)$ , where  $2\pi/p$  is the period, of the same type as one recently deduced by von Helmholtz by an abstract process based on the principle of Least Action combined with a theory of electrons, which, however, does not correspond with the views here developed. That this formula is a good representation of the experimental facts for ordinary transparent media is generally recognised; especially as it may, in case of necessity, be modified by the inclusion of slight non-selective opacity, due to drift of free electrons, after the manner of ordinary conduction. When this kind of general opacity is predominant, the result obtained in the paper conforms to the main features of metallic propagation; thus, with sufficient conductivity the real part of the square of the refractive index becomes negative, and the real part of the index itself may become less than unity, while the dispersion is usually abnormal.

When the phenomena of moving media are treated, dispersion may, for simplicity, be left out of account. It is shown that, if the view described in the previous paper, that all the dynamical properties of matter are to be derived from the relations of electrons, with or

without intrinsic inertia, in a rotationally elastic fluid æther, is ascertained, the null result of the Michelson-Morley second-order experiment on the effect of the Earth's motion on the velocity of light becomes included in the theory; in fact, according to a suggestion brought out by FitzGerald and Lorentz, and developed somewhat in this manner by the latter, the second-order optical effect is just compensated by a second-order effect on the lengths of the moving arms of Michelson's apparatus, which is produced by its motion along with the Earth through the æther.

As mixed dynamical and statistical theories of electrons or other objects require delicate treatment, especially when pushed, as here, to the second order of small quantities, the formulæ of this part of the paper are deduced independently by two very different analytical methods. In the first place, there is the usual process of extending the fundamental circuital relations of the free æther which express its dynamical relations as differential equations of the first order, by suitable modification of the significance of the vectors involved in them, so that the same equations shall apply to ponderable media as well, the vectors then representing averages taken over the element of volume. The other method consists in working out the dynamics of a single electron, and applying the results statistically to the inclusion of the various ways in which the electric current arises from the movements of the electrons in ponderable media.

The theory as thus developed from the electron as the fundamental element, may be stated in a form which is independent of the dynamical hypothesis of a rotational æther. Maxwell's formal equations of the electric field may take the place of that hypothesis, though it may, I think, be contended that an abstract procedure of that kind will neither be so simple nor so graphic, nor lend itself so easily to the intuitive grasp of relations, as a more concrete one of the type here employed.

The exact permanence of the wave-lengths in spectra, under various physical conditions, may be ascribed to the influence of radiation on the molecule, which keeps it in, or very close to, a constant condition of steady motion, of minimum total energy corresponding to its pre-determined constant momenta. It is also pointed out, from the analogy of physical astronomy, that the harmonic oscillations into which the spectroscopist divides the radiation from a molecule, may be far more numerous than the co-ordinates which specify its relative motions; that, therefore, relations of a semi-dynamical character may be discovered among the spectral lines, without its being rendered likely that we can ever penetrate from them back to the actual configuration of the molecular system.

Reverting finally to matters relating purely to a rotational æther theory, with electrons as the sole foundation for matter, it is possible to

identify the inertia of matter with the electric inertia of the electrons, if only we may assume their nuclei to be small enough, or sufficiently numerous. And the fact that these nuclei have free periods of elastic radial vibration in the fluid æther, not subject to damping by radiation, reminds us that a pulsatory theory of gravitation has been developed by Hicks and Bjerknes. There is no recognised fundamental interaction of electric and radiative phenomena with gravitation, so for present purposes we are not bound to produce a precise explanation of gravitation at all. The scope of this remark is restricted to merely showing that a rotational æther is not incompetent to include such an action among its properties.

III. "On the Refractive Index of Water at Temperatures between  $0^{\circ}$  and  $10^{\circ}$ ." By Sir JOHN CONROY, Bart., F.R.S., Fellow and Bedford Lecturer of Balliol College, and Millard Lecturer of Trinity College, Oxford. Received May 16, 1895.

In 1856 Jamin ('Comptes Rendus,' vol. xliii, p. 1191) published an account of observations he had made on the refractive index of water at temperatures between  $30^{\circ}$  and  $0^{\circ}$ . He used an interference method, and found that as the water cooled the index increased.

"La masse totale de l'eau qui d'abord était à 12 degrés, se refroidissant continuellement, arriva bientôt à 4 degrés, c'est-à-dire, au point où les variations de l'indice devaient changer de signe, et où le déplacement des franges devrait être inverse. Mais rien de pareil ne se montra, et en continuant le refroidissement jusqu'à zéro, on continua d'observer une augmentation de l'indice. Il n'y a donc pas de maximum dans la valeur du coefficient de réfraction quand il y en a un dans la densité."

In another experiment the temperature of the column of water through which one of the beams of light passed was kept at  $0^{\circ}$ , whilst that of the other was gradually raised to  $30^{\circ}$ ; he found by the displacement of the bands that the index decreased steadily. He did not, apparently, publish any numerical values for the indices, but states that they are accurately given by the empirical formula  $K_t = K_0 - (0.000012573)t - (0.000001929)t^2$ .

Two years later Gladstone and Dale ('Phil. Trans.,' 1858, p. 887) gave an account of observations that they had made "on the influence of temperature on the refraction of light;" they used a hollow glass prism, and determined the angles of minimum deviation for water, and several other liquids, at various temperatures. They say, "our determinations were performed repeatedly and most carefully on water near the freezing point; they confirm the observations

of the French physicist" (Jamin) "but show at the same time that the remarkable reversion of the density at 4°, is not without its influence on the amount of sensitiveness; the change of refractive index between 10° and 5° being 0.0002, whilst that between 5° and 0° is only 0.0001."

They give a table for the values of the index to five places, for A, D, and H at eight temperatures between 0° and 11°.

In 1867 Rühlmann ('Pogg. Ann.,' vol. 132, p. 1 and 176) published an account of observations he had made of the refractive index of water at various temperatures; he also used a hollow glass prism and gives the values to five places, for lithium, sodium, and thallium light from 0° to 100°. He states, "Der Brechungsindex des Wassers nimmt stetig ab von 0° bis 80° R., ohne bei dem Dichtigkeitsmaximum irgend eine Abweichung von dem Aenderungsgesetze zu zeigen, mithin die Fortpflanzungsgeschwindigkeit des Lichtes stetig zu."

Lorenz ('Wied. Ann.,' vol. 11 [1880], p. 70) made observations by an interference method, on the refractive index of water between the temperatures of 0° and 34°; and Dufet ('Jour. de Physique' (2), vol. 4 [1885], p. 389) determined the index of water at temperatures above 17° by the minimum deviation method and by an interference method, and also calculated from the results obtained by other observers the rate of change of the index with change of temperature.

Ketteler ('Wied. Ann.,' vol. 33 [1888], pp. 353 and 506) repeated Rühlmann's determinations for temperatures above 20°, using a total reflection refractometer, but did not make any observations at lower temperatures. More recently still, B. Walter has published ('Wied. Ann.,' vol. xlv [1892], p. 422) a short account of some determinations of the refractive index of water to five places between 0° and 30° for the D line, made, apparently, with great care by the minimum deviation method.

That the refractive index of water increases with the decrease of temperature until the freezing point is reached, appears to be proved, but as few determinations of the values of the refractive indices of water near its point of maximum density have been published, I have ventured, as the matter is one of considerable theoretical importance, to bring before the Society an account of some determinations I have recently made.

The method employed was the ordinary one, the determination of the angle of minimum deviation for a ray of definite wave-length passing through a hollow glass prism containing water at a known temperature.

The goniometer used was made by Messrs. Troughton and Simms, it has an 8-inch circle divided into 10', and is read by means of two micrometers, directly to 10', and by estimation to single seconds.

The prism was made by Steinheil; the value of its refracting angle, as determined by six independent measurements, was  $60^{\circ} 1' 42'' \pm 0.8''$ .

The prism was surrounded by a water-jacket, through which a stream of brine, cooled by a freezing mixture, could be passed.

Openings in the water-jacket allowed the light which had passed through the collimator to reach the prism, and the refracted beam to reach the telescope. The temperature was ascertained by means of a thermometer with its bulb immersed in the water contained in the prism.

The prism was filled with distilled water which had been recently boiled and allowed to cool under reduced pressure.

The determinations were made exclusively with sodium light; it had been originally intended to make observations with lights of different refrangibilities, but it was found that, owing to the brilliancy and constancy of the sodium light, it was not only far easier to make observations with it, but that these observations would certainly be more accurate than those made with light of other refrangibilities.

The prism, not being in actual contact with the water-jacket, cooled very slowly, about four or five hours were usually necessary to reduce its temperature from about  $9^{\circ}$  to a little above the freezing point.

Owing to the experiments being made at temperatures different from that of the room, and to the temperature of the prism continually, though slowly, altering, it was found impossible to make the determinations by reversing the prism, and then taking half the angle between the two positions of the telescope as the angle of minimum deviation.

Several series of observations were therefore made with the prism in both positions of minimum deviation, and the differences between these readings and those made when the axes of the collimator and telescope were in the same straight line, gave the deviations.

Seven sets of observations were made, and the results are contained in Table I; which gives the deviations for both positions of the prism, and the corresponding refractive indices for the various temperatures.

The angles of deviation differ so little from each other, that any error in the determination of the refracting angle of the prism would not make any difference in the relative values of the indices; it would, of course, affect their absolute values. The probable error calculated by the ordinary formula  $\left(0.674 \sqrt{\frac{\sum d^2}{n(n-1)}}\right)$  from the measurements made of the angle of the prism (see above) is  $\pm 0.8''$ . A difference of  $1''$  in the value of the refracting angle corresponds to two units in the sixth place in the refractive index; the probable

Table I.

| Base of prism to right. |             |                   | Base of prism to left. |             |                   |
|-------------------------|-------------|-------------------|------------------------|-------------|-------------------|
| <i>t.</i>               | Deviation.  | Refractive index. | <i>t.</i>              | Deviation.  | Refractive index. |
| 9.1                     | 23° 39' 58" | 1.333722          | 8.7                    | 23° 40' 18" | 1.333794          |
| 8.7                     | 23 40 08    | 1.333758          | 7.8                    | 23 40 26    | 1.333823          |
| 8.3                     | 23 40 06    | 1.333751          | 7.6                    | 23 40 25    | 1.333820          |
| 7.7                     | 23 40 14    | 1.333780          | 7.3                    | 23 40 26    | 1.333823          |
| 7.1                     | 23 40 19    | 1.333798          | 6.7                    | 23 40 35    | 1.333856          |
| 6.6                     | 23 40 22    | 1.333809          | 6.3                    | 23 40 37    | 1.333863          |
| 6.2                     | 23 40 25    | 1.333820          | 6.2                    | 23 40 34    | 1.333822          |
| 5.5                     | 23 40 32    | 1.333845          | 5.6                    | 23 40 41    | 1.333877          |
| 4.9                     | 23 40 38    | 1.333866          | 5.4                    | 23 40 47    | 1.333899          |
| 4.3                     | 23 40 47    | 1.333905          | "                      | 23 40 57    | 1.333935          |
| 3.7                     | 23 40 46    | 1.333895          | 4.9                    | 23 40 49    | 1.333907          |
| 3.6                     | 23 40 49    | 1.333906          | "                      | 23 40 48    | 1.333902          |
| "                       | 23 40 51    | 1.333913          | 4.8                    | 23 40 59    | 1.333942          |
| 3.3                     | 23 40 51    | 1.333913          | 4.1                    | 23 40 51    | 1.333913          |
| 2.9                     | 23 40 58    | 1.333939          | "                      | 23 40 55    | 1.333928          |
| "                       | 23 40 54    | 1.333924          | 3.9                    | 23 40 55    | 1.333928          |
| 2.8                     | 23 40 55    | 1.333928          | 3.6                    | 23 40 53    | 1.333890          |
| 2.7                     | 23 40 49    | 1.333906          | "                      | 23 40 54    | 1.333924          |
| 2.4                     | 23 40 59    | 1.333942          | 3.2                    | 23 41 04    | 1.333960          |
| 2.3                     | 23 40 59    | 1.333942          | "                      | 23 40 53    | 1.333920          |
| 2.1                     | 23 40 52    | 1.333917          | 3.1                    | 23 40 57    | 1.333935          |
| 2.0                     | 23 41 0     | 1.333946          | 2.9                    | 23 40 55    | 1.333928          |
| "                       | 23 41 0     | 1.333946          | 2.8                    | 23 41 02    | 1.333963          |
| 1.7                     | 23 41 0     | 1.333946          | 2.5                    | 23 40 59    | 1.333942          |
| 1.5                     | 23 41 0     | 1.333946          | 2.3                    | 23 41 08    | 1.333975          |
| 1.3                     | 23 40 55    | 1.333928          | "                      | 23 41 03    | 1.333957          |
| 1.2                     | 23 41 07    | 1.333971          | 2.2                    | 23 41 04    | 1.333960          |
| 1.0                     | 23 41 06    | 1.333967          | 2.1                    | 23 41 0     | 1.333946          |
| 0.9                     | 23 41 19    | 1.334015          | 1.9                    | 23 40 59    | 1.333942          |
| "                       | 23 41 14    | 1.333996          | 1.8                    | 23 41 04    | 1.333960          |
| 0.8                     | 23 41 06    | 1.333967          | 1.7                    | 23 40 56    | 1.333931          |
| "                       | 23 41 05    | 1.333964          | 1.5                    | 23 40 57    | 1.333935          |
| 0.7                     | 23 41 0     | 1.333946          | 1.4                    | 23 41 01    | 1.333950          |
| 0.6                     | 23 41 20    | 1.334018          | "                      | 23 41 05    | 1.333964          |
| "                       | 23 41 16    | 1.334004          | 1.2                    | 23 41 06    | 1.333967          |
| 0.3                     | 23 41 16    | 1.334004          | 1.0                    | 23 41 10    | 1.333982          |
| "                       | 23 41 19    | 1.334015          | "                      | 23 41 03    | 1.333957          |
| "                       | 23 41 0     | 1.333946          | 0.6                    | 23 41 02    | 1.333953          |
| 0.2                     | 23 41 20    | 1.334018          | "                      | 23 41 04    | 1.333960          |
|                         |             |                   | 0.5                    | 23 40 58    | 1.333939          |
|                         |             |                   | 0.4                    | 23 41 04    | 1.333960          |
|                         |             |                   | "                      | 23 41 0     | 1.333946          |
|                         |             |                   | 0.2                    | 23 41 06    | 1.333967          |

error in the refractive indices due to the measurement of the refracting angle of the prism is therefore rather less than  $\pm 0.000002''$ .

A difference of 1" in the value of the angle of minimum deviation

corresponds to 4 units in the sixth place of the refractive index; as the sets of micrometer readings were all fairly concordant, and their probable errors less than 1", the values of the indices were calculated to six places.

The temperatures at which the observations were made not being identical for the two positions of the prism, it was thought that a graphical method, though, from its nature, somewhat "arbitrary," would give a more truthful result than any arithmetical process of taking the means. The results were therefore plotted on paper divided into squares of 1 mm., 0.2° of temperature being represented by 5 mm. on the axis of abscissæ, 0.00001 of refractive index by 4 mm. on that of the ordinates; and a curve drawn in the ordinary way.

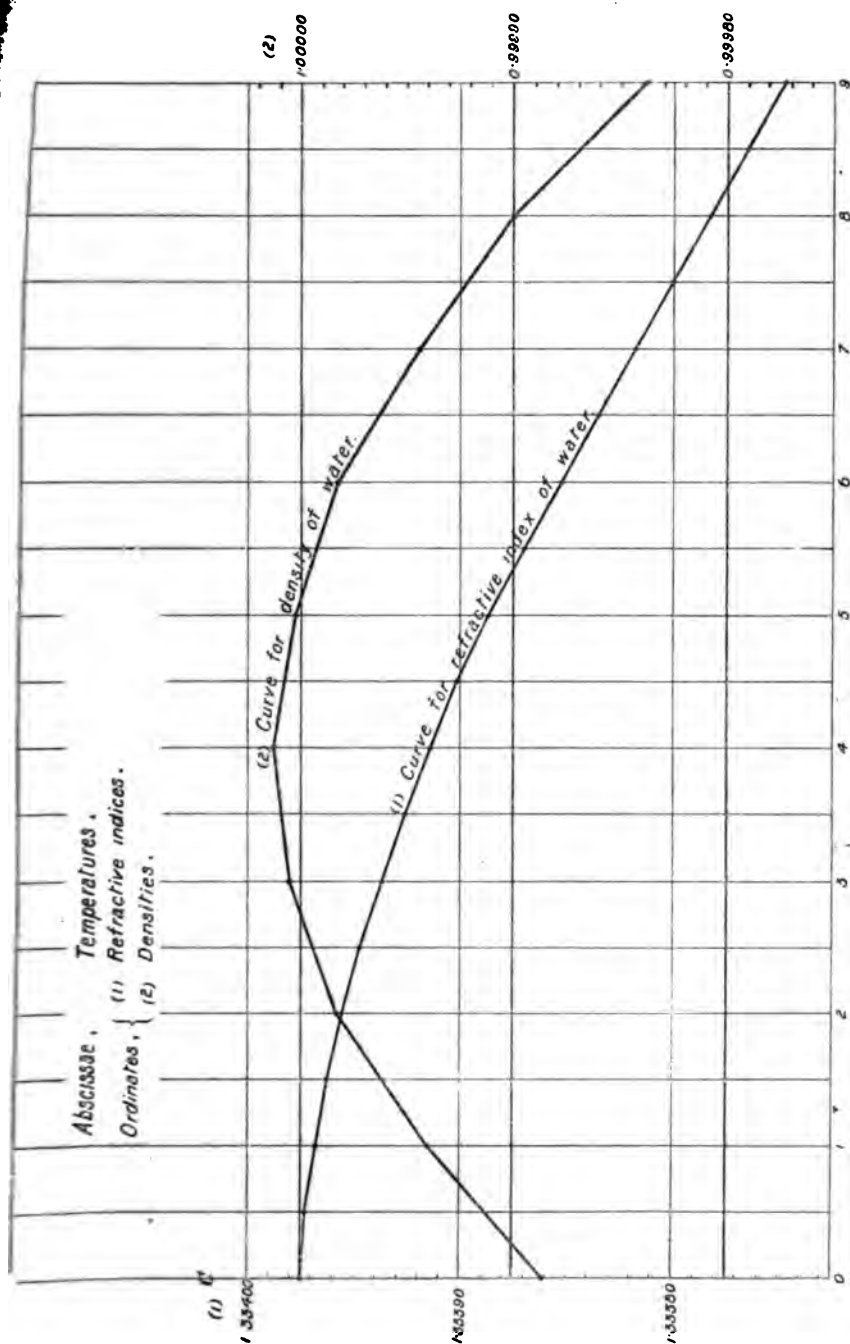
Owing to the scale on which the results were plotted, a good deal of "judgment" was necessary in drawing the curve. It was therefore thought desirable also to plot the results on a smaller scale, so far as the ordinates were concerned; this was done, one unit in the fifth place being represented by 2 mm., and another curve drawn. The values of the index for different temperatures, as given by the two curves were compared and were found to agree satisfactorily; and it therefore seemed probable that the curves were really fair representations of the observations.

In the first column of Table II the values of the refractive indices, relative to air, for each degree as deduced from the curves, are given to five places; in the second the values as found by Walter, and in the third and fourth those for sodium light, given by Gladstone and Dale, and Rühlmann.

Table II.—Refractive Indices of Water.

| <i>t.</i> | C.      | W.      | <i>t.</i> | G and D. | <i>t.</i> | R.        |
|-----------|---------|---------|-----------|----------|-----------|-----------|
| 0°        | 1.33397 | 1.33401 | 0.0°      | 1.33374  | 0.0°      | { 1.33375 |
| 1         | 1.33397 | 1.33400 | 4.0       | 1.33367  |           | { 1.33380 |
| 2         | 1.33396 | 1.33398 | 6.5       | 1.33356  | 1.5       | 1.33375   |
| 3         | 1.33394 | 1.33396 | 9.0       | 1.33342  | 4.0       | 1.33372   |
| 4         | 1.33392 | 1.33393 | —         | —        | 5.0       | 1.33371   |
| 5         | 1.33389 | 1.33390 | —         | —        | 5.8       | 1.33368   |
| 6         | 1.33385 | 1.33387 | —         | —        | 9.9       | 1.33355   |
| 7         | 1.33382 | 1.33383 | —         | —        | 10.0      | 1.33353   |
| 8         | 1.33378 | 1.33379 | —         | —        | —         | —         |
| 9         | 1.33375 | 1.33374 | —         | —        | —         | —         |

The values show that the refractive index of water, as was first announced by Jamin, increases continuously up to the freezing point, the rate of increase, however, seems to change about 4°, the temperature of maximum density, as was pointed out by Gladstone and Dale.



Curve (1) gives the values of the refractive indices at temperatures between  $0^{\circ}$  and  $9^{\circ}$ , as determined by the experiments, of which an account has been given in this paper; and curve (2) the densities of water at the same temperatures as given in Lupton's 'Numerical Tables' (p. 28).

These curves show clearly that no formula representing the variation of the refractive index of water with the temperature, as a function of the density only, can be a complete expression of the facts of the case.

- IV. "On the Magnetic Rotation of the Plane of Polarisation of Light in Liquids. Part I. Carbon Bisulphide and Water."  
By J. W. RODGER, Assoc.R.C.S., Demonstrator of Chemistry, and W. WATSON, B.Sc., Assoc.R.C.S., Demonstrator of Physics, Royal College of Science, London. Communicated by Professor A. W. RÜCKER, F.R.S. Received June 1, 1895.

(Abstract.)

The aim of this investigation is the determination in absolute measure of the magnetic rotation of liquids at different temperatures, the effect of the chemical nature of the liquid on this property, and its correlation with other physical properties.

The present communication contains a description of the apparatus and method of experiment, and the results obtained with the standard liquids, carbon bisulphide and water, for sodium light, in a magnetic field of constant intensity, and at different temperatures between  $0^{\circ}$  and the ordinary boiling point.

The magnetic field was produced by means of a helix consisting of two separate coils, either of which, if desired, could be used separately. During the process of winding, the dimensions of each layer were carefully determined, and every precaution was taken to ensure good insulation.

The liquid under examination was contained in a glass tube closed by very thin glass plates. This tube was surrounded by a brass jacket, which passed through the coils. Observations could be made at different temperatures, and the temperature could be kept constant while making a set of readings by causing water, or the vapour of liquids boiling under different pressures, to circulate through the jacket.

Special pains were taken to obtain monochromatic light, with the result that in the case of carbon bisulphide, where the double rotation amounted to  $40^{\circ}$ , there was no trace of coloration.

The ultimate standard of current was a silver voltameter, a Kelvin deci-ampère balance being used as an intermediate standard.

In the case of carbon bisulphide three different samples were used, and identical results were obtained with three separate coils. In the following table are collected the mean values of the boiling point (b. p.), density at 0° ( $\rho_0$ ), and Verdet's constant at 0° ( $\gamma_0$ ). Verdet's constant may be defined as the rotation in minutes of arc produced in a column of liquid when the difference between the magnetic potentials at the ends of the column is equal to one C.G.S. unit.

|                             | B. p. | $\rho_0$ . | $\gamma_0$ . |
|-----------------------------|-------|------------|--------------|
| CS <sub>2</sub> No. 1 ..... | 46°25 | 1·29271    | 0·04348      |
| CS <sub>2</sub> No. 2 ..... | 46°26 | 1·29282    | 0·04347      |
| CS <sub>2</sub> No. 3 ..... | 46°26 | 1·29283    | 0·04347      |

It will be seen that the three different samples give practically identical values for the three physical constants.

The results obtained for the rotation of carbon bisulphide may be summed up in the following equation, where  $\gamma_t$  is the value of Verdet's constant at the temperature  $t$ ,

$$\gamma_t = 0\cdot04347 (1 - 0\cdot001696t),$$

or

$$\gamma_t = 0\cdot04347 - 0\cdot0_4737t.$$

The expression connecting rotation and temperature is therefore linear.

In the case of water the results are best represented by

$$\gamma_t = 0\cdot01311 (1 - 0\cdot0_4305t - 0\cdot0_4305t^2),$$

or

$$\gamma_t = 0\cdot01311 - 0\cdot0_44t - 0\cdot0_44t^2.$$

Here the rate of change of the rotation with temperature increases as the temperature rises.

The following table gives the values of the rotation of water and carbon bisulphide at every 10° between 0° and the boiling point, as well as the values of the quotient ( $\gamma/\rho$ ), obtained by dividing the rotation by the density.

In the case of water the quotient  $\gamma/\rho$  is practically constant up to 20°, it then very slowly *increases*, the rate of increase between 20° and 100° being practically constant.

For carbon bisulphide the quotient  $\gamma/\rho$  *decreases* at a constant rate as the temperature rises, the rate of decrease being very much greater than the rate of increase in the case of water.

| <i>t.</i> | Water.     |                 | Carbon bisulphide. |                 |
|-----------|------------|-----------------|--------------------|-----------------|
|           | $\gamma$ . | $\gamma/\rho$ . | $\gamma$ .         | $\gamma/\rho$ . |
| 0         | 0·01311    | 0·01311         | 0·04347            | 0·03362         |
| 10        | 0·01310    | 0·01311         | 0·04273            | 0·03344         |
| 20        | 0·01309    | 0·01311         | 0·04200            | 0·03325         |
| 30        | 0·01306    | 0·01312         | 0·04126            | 0·03307         |
| 40        | 0·01303    | 0·01313         | 0·04053            | 0·03288         |
| 50        | 0·01299    | 0·01315         |                    |                 |
| 60        | 0·01294    | 0·01316         |                    |                 |
| 70        | 0·01289    | 0·01318         |                    |                 |
| 80        | 0·01282    | 0·01319         |                    |                 |
| 90        | 0·01275    | 0·01321         |                    |                 |
| 100       | 0·01267    | 0·01322         |                    |                 |

The measure of the molecular rotation which is usually employed in chemical investigations is

$$(\mathbf{M}\gamma/\rho)_{\text{substance}}/(\mathbf{M}\gamma/\rho)_{\text{water}},$$

where *M* is the molecular weight. Although the authors postpone a detailed discussion of the validity of this expression, they show that for carbon bisulphide, at any rate, its value changes with the temperature, and hence the conclusions obtained by its use regarding questions of chemical constitution, especially of tautomerism, are affected on this account.

They also point out that the above expression involves the properties of water. The only justification for the use of water in relative observations is the elimination of variations in the strength of the magnetic field in which the observations are made. If the temperature of observation is always the same, this can readily be done. If, on the other hand, the temperature varies, it is essential to know how the rotation of water alters with the temperature. In the past this alteration was unknown, and the arbitrary measure of the molecular rotation above referred to has come into use. Since an expression for the temperature variation has now been obtained it is to be hoped that observers will employ a measure of the molecular rotation which does not involve the properties of water. Indeed, other considerations make such a measure all the more desirable. Up till now the authors have made observations on eight liquids, besides water and carbon bisulphide, and in all cases except that of water the relation between rotation and temperature is linear, and the quotient, rotation divided by density, diminishes as the temperature rises. It is highly probable, therefore, that as regards magnetic rotation, as in the case of so many other properties, the

behaviour of water is exceptional, and hence it is particularly ill suited for the use to which it has been put. Again, on account of the smallness of the rotation in water, the unavoidable inaccuracies in determining its rotation, and thus estimating the strength of the magnetic field, produce a larger percentage error in the results than if a liquid, such as benzene, having a considerably higher rotation than water, were used for this purpose.

V. "The Influence of the Cerebral Cortex on the Larynx."

By J. S. RISIEN RUSSELL, M.D., M.R.C.P., Assistant Physician to the Metropolitan Hospital. Communicated by Professor VICTOR HORSLEY, F.R.S. Received June 5, 1895.

(From the Pathological Laboratory of University College, London.)

(Abstract.)

This research was undertaken in order to attempt to differentiate an abductor centre in the cerebral cortex of the dog, but, as several new observations were made during the course of the investigation, similar experiments were performed in the cat in order to compare the results obtained in this animal with those obtained in the dog.

Among the preliminary considerations was the question as to what influence the condition of the peripheral laryngeal apparatus in an animal has on the effects obtained from its central nervous mechanism, and the conclusion arrived at was that the influence must be comparatively insignificant, as abduction or adduction of the vocal cords could be obtained on excitation of the appropriate area of the cerebral cortex, irrespective of whether abduction or adduction was obtained on excitation of the recurrent laryngeal nerves in the same animal.

No evidence of unilateral representation of the movements of the vocal cords in the cerebral cortex was obtained; and in testing this point one recurrent laryngeal nerve was divided transversely, when it was found possible to influence the vocal cord whose nerve was intact, with equal ease on stimulation of either cerebral hemisphere.

The question of inhibition of antagonistic muscles by electrical excitation of the cerebral cortex, on the lines adopted by Sherrington with regard to antagonistic muscles in other parts of the body, was tested by first dividing the adductor fibres in both recurrent laryngeal nerves, leaving the abductor fibres intact, and then exciting the adductor centre with strong induced currents; but no evidence of inhibition of the abductor muscles was obtained.

The major part of the paper deals with the movements of the vocal cords which could be evoked on excitation of different foci in the cerebral cortex. It was found that both in the dog and cat there

existed a focus, excitation of which resulted in adduction of the vocal cords, and another near to this, stimulation of which resulted in abduction of the cords. While in the cat it was possible to differentiate these movements without any preliminary measures being adopted, it was otherwise in the dog, for it was only after the adductor fibres of one recurrent laryngeal nerve had been divided transversely that it first became possible to evoke abduction of the vocal cords on excitation of the cortex, though in subsequent experiments it was sometimes possible to evoke this movement on excitation of the cortex of the dog without adopting this preliminary measure. The other effect on the cords, which it was as a rule found most difficult to differentiate from that of abduction, was acceleration of their movements.

In further exploring the cortex it was found that on the anterior composite gyrus, below the abductor centre, there existed a focus, excitation of which resulted in what is described as a clonic adductor effect on the cords, in which the cords were first brought into a position of moderate adduction, and then there was added rapid short to-and-fro excursions.

On passing within the confines of Spencer's area for arrest of respiration, it was found that in the peripheral parts of this area there existed three foci, excitation of which affected the cords in different ways. The most anterior of these foci was responsible for arrest of the cords in adduction, *i.e.*, in the expiratory stage of their excursions; excitation of the focus behind this, and corresponding, probably, to Horsley and Semon's abductor centre in the cat, was followed by arrest of the cords in abduction, *i.e.*, their inspiratory position; while the most posterior focus, which is situated at about the junction of the anterior composite and anterior sylvian convolutions, resulted in intensification combined with acceleration of the movements of the cords when stimulated. Excitation of Spencer's chief focus for arrest of respiration on the olfactory lobe, resulted in arrest of the cords in the position they occupy during expiration in dogs, and in the position they occupy during inspiration in cats.

VI. "An Enquiry into the Nature of the Vesicating Constituent of Croton Oil." By WYNDHAM R. DUNSTAN, M.A., F.R.S., and Miss L. E. BOOLE, F.I.C., Lecturer on Chemistry in the London School of Medicine for Women. Received June 5, 1895.

(Abstract.)

The vesicating constituent, or more strictly, the pustule-producing constituent of croton oil, has been the subject of investigation by

numerous chemists and pharmacologists during the past forty years. According to the researches of Buchheim, and more recently of Kobert and Hirscheydt, the vesicating action is due to an acid closely allied to oleic acid, which has been given the name of crotonoleic acid. This substance is now prepared on a large scale in Germany for medical use, being extracted from croton oil by the method devised by Kobert and Hirscheydt. This consists in saponifying with barium hydroxide that part of croton oil which readily dissolves in strong alcohol. The resulting barium salts are washed with water, then dried, and repeatedly extracted with ether, which dissolves the barium salts of oleic and crotonoleic acids. These salts are separated by means of ether, which dissolves only the barium crotonoleate, and this, when decomposed with dilute sulphuric acid and extracted with ether, furnishes the crotonoleic acid as a viscid oil.

Since very little is known about this acid, even its composition being undetermined, the authors prepared it with the object of studying its properties and, if possible, of determining the constitution, since no fatty acid of known constitution exhibits the property of vesicating. Starting with the crotonoleic acid prepared as described above, the lead salt was obtained and submitted to a process of fractional precipitation by adding successive quantities of water to its solution in alcohol. By this means crotonoleic acid was proved to be a mixture composed for the most part of inactive oily acids, the lead salts of which are precipitated first, whilst the true vesicating constituent (or its lead salt) is principally contained in the last fractions, and represents but a small proportion of the original material. It was observed that the conversion of the crotonoleic acid into a lead salt did appreciably affect its vesicating power.

The supposed active constituent of croton oil, crotonoleic acid, having thus been shown to be a mixture, the authors proceeded to attempt to isolate the vesicating constituent from croton oil direct.

By saponifying that part of croton oil which is soluble in strong alcohol with a mixture of lead oxide and water, and repeatedly fractionating an alcoholic solution of the lead salts with water, the later fractions, which possessed the greatest vesicating power, ultimately furnished, when submitted to a series of fractionations, a resinous substance having extraordinary power as a vesicant. This substance could not be further resolved by repeating the process of fractional precipitation of the alcoholic solution with water. The same substance was isolated from the so-called "crotonoleic acid," and the authors propose to name it "croton-resin." To its presence the vesicating property of croton oil is due. The composition of croton-resin is expressed by the empirical formula  $C_{15}H_{18}O_4$ . So far all attempts to crystallise it, or to obtain crystalline derivatives from it,

have been unsuccessful. It is a hard, pale yellow, brittle resin, nearly insoluble in water, light petroleum, and benzene, but readily dissolved by alcohol, ether, and chloroform. When heated it gradually softens, and is quite fluid at  $90^{\circ}$  C. Croton-resin has neither basic nor acidic properties; it may be boiled with a mixture of lead oxide and water without being appreciably affected. Ebullition with aqueous potash or soda gradually decomposes it, destroying its vesicating power. The products of this action are several acids, some of which are members of the acetic series. By oxidation of the resin with nitric acid a mixture of acids is obtained. The constitution of croton-resin is therefore complicated, and its molecular formula would appear to be at least  $(C_{12}H_{16}O_4)_2$  or  $C_{24}H_{32}O_8$ . Since it is not saponified by a mixture of lead oxide and water, and as no glycerol could be detected among the products of its decomposition by alkalis, it is not a glyceride, and as it does not react with hydroxylamine or phenylhydrazine or sodium bisulphite, it is probably neither a ketone nor an aldehyde. The evidence so far obtained points to the conclusion that the constitution of the vesicating constituent of croton oil is that of a lactone or anhydride of complicated structure.

VII. "Note on Regression and Inheritance in the Case of 'Two Parents.'" By KARL PEARSON, University College, London. Communicated by FRANCIS GALTON, F.R.S. Received June 5, 1895.

Consider a population in which sexual selection and natural selection may or may not be taking place. Assume only that the deviations from the mean in the case of any organ of any generation follow exactly or closely the normal law of frequency, then the following expressions may be shown to give the law of inheritance of the population.

Let  $H_1$  = deviation of the father from mean of the total population of fathers, and let  $\sigma_1$  = the standard deviation of the total population of fathers.

Let  $H_2$  and  $\sigma_2$  be the corresponding quantities for the mother for the same (or any other) organ. Let  $H_3$  be the mean deviation of the offspring (fraternity) due to fathers  $H_1$  and mothers  $H_2$  with regard to the same (or any other) organ, and let  $\sigma_3$  be the standard deviation of the whole offspring population. If we are dealing with offspring of one sex, both the mean and the standard deviation of the offspring may differ, owing to natural selection, by any amount from those of parents of the same sex.

Let  $r_1$  = co-efficient of correlation (Galton's function\*) for the two organs (or same organ) of the parent population, i.e.,  $r_1$  is a measure of the amount of sexual selection between the parents of the population with regard to these organs.

Let  $r_2$  = co-efficient of correlation between fathers and offspring for the organ (or two organs) under consideration, i.e.,  $r_2$  is a measure of the paternal inheritance.

Let  $r_3$  = co-efficient of correlation between mothers and offspring for the organ (or two organs) under consideration, i.e.,  $r_3$  is a measure of the maternal inheritance.

The value of  $H_1$  is given by

$$H_1 = \frac{r_2 - r_1 r_3}{1 - r_1^2} \frac{\sigma_1}{\sigma_2} H_2 + \frac{r_3 - r_1 r_2}{1 - r_1^2} \frac{\sigma_1}{\sigma_3} H_3,$$

and the standard deviation  $\Sigma$  of the fraternity due to parents  $H_2$  and  $H_3$  is given by

$$\Sigma = \frac{\sigma_1}{\sqrt{(1 - r_1^2)/(1 - r_1^2 - r_2^2 - r_3^2 + 2r_1 r_2 r_3)}}.$$

Thus the distribution of fraternities is the same for all parentages; it depends, however, upon the strength of sexual selection, and on the paternal and maternal inheritances for the community at large with regard to the organs under consideration.

The portion of regression due to either parent alone is not dependent solely on maternal or paternal inheritance; it is influenced not only indirectly by sexual selection but directly by the inheritance from the other parent owing to the presence of the terms  $r_1 r_2$  and  $r_1 r_3$ . Further, the greater the variability of one sex (i.e., the greater  $\sigma_2$  or  $\sigma_3$ ) the less, other things being the same, the parent of that sex contributes to the inheritance of the offspring. The above two formulæ seem to embrace the chief laws of heredity in populations. The whole of the constants involved can be found by comparatively simple measurements, and, indeed, have been, to some extent, found in the case of man by Mr. Galton.†

\* The probable error of a determination of Galton's function

$$= 0.674506 \frac{1 - r_1^2}{\sqrt{n(1 + r_1^2)}},$$

where  $n$  is the total number of correlated pairs. Mr. Galton having kindly placed at my disposal his 'Family Faculty Records,' I find that  $r_1$  for height is, as he supposed, small, = 0.093. But the probable error of the determination ( $n = 198$  only) is 0.047. Hence the balance of probability is in favour of a certain small amount of sexual selection as to height in human marriage. I hope shortly to have sufficient data to confirm this result.

† They do not seem, however, to fully justify his theory of the midparent. I hope at a later date to discuss its special limitations, e.g.,  $\sigma_2$  and  $\sigma_3$  differ considerably in several series of skull measurements with which I have had to deal.

These results are taken from a longer paper on panmixia and regression, which ill-health has prevented my completing up to the present date.

VIII. "On the Occlusion of Oxygen and Hydrogen by Platinum Black. (Part I.)" By LUDWIG MOND, F.R.S., WILLIAM RAMSAY, Ph.D., F.R.S., and JOHN SHIELDS, D.Sc., Ph.D. Received June 13, 1895.

(Abstract.)

The authors describe some preliminary experiments on the occlusion of oxygen and hydrogen by platinum sponge and foil, which in general confirm the results obtained by Graham. At most only a few volumes of these gases are occluded by the more coherent forms of platinum.

After giving details of what they consider the best method of preparation of platinum black, they next describe some experiments which had for their object the determination of the total quantity of water retained by platinum black, dried at 100° C., and the amount of water which can be removed from platinum black at various temperatures in vacuo. As the result of these experiments they find that platinum black dried at 100° retains in general 0.5 per cent. of water, and this can only be removed in vacuo at a temperature (about 400°) at which the black no longer exists as such, but is converted at least partially into sponge. At any given temperature the water retained by platinum black seems to be constant. The density of platinum black dried at 100° C. is 19.4, or allowing for the water retained by it at this temperature, 21.5.

The amount of oxygen given off by platinum black at various temperatures was determined. Altogether it contains about 100 volumes of oxygen; the oxygen begins to come off in quantity at about 300° C. in vacuo, and the bulk of it can be extracted at 400° C., but a red heat is necessary for its complete removal. Small quantities of carbon dioxide were also extracted, chiefly between 100—200° C.

In determining the quantity of hydrogen occluded by platinum black the authors have carefully distinguished between the hydrogen which goes to form water with the oxygen always contained in platinum black, and that which is really absorbed by the platinum *per se*. Altogether about 310 volumes of hydrogen are absorbed per unit volume of platinum black, but of this 200 volumes are converted into water, or only 110 volumes are really occluded by the platinum. Part of it can be again removed at the ordinary temperature in vacuo; by far the larger portion can be extracted at about 250—300° C., but a red heat is necessary for its complete removal. The amount of hydro-

gen absorbed by platinum is very largely influenced by slight traces of impurity, probably grease or other matter which forms a skin over the platinum.

Platinum black in vacuo absorbs a certain quantity of hydrogen. On increasing the pressure of the hydrogen up to about 200–300 mm. a further quantity is absorbed, but after this pressure is almost without effect. By increasing the pressure from one atmosphere up to four and a half atmospheres, only one additional volume of hydrogen was absorbed. On placing platinum black charged with oxygen in an atmosphere of oxygen, and increasing the pressure to the same extent eight and a half additional volumes were however absorbed.

Platinum black charged with hydrogen and placed in an atmosphere of hydrogen kept approximately at atmospheric pressure, and platinum black charged with oxygen and confined in an atmosphere of oxygen, behave quite differently when heated. In the former case hydrogen is immediately expelled on raising the temperature, whilst in the latter case oxygen is steadily absorbed until a temperature of about 360° C. (the temperature of maximum absorption) is reached, when on further heating oxygen begins to come off again.

Incidentally it was noticed that mercury begins to combine with oxygen at 237° C., and that a mixture of platinum black and phosphorus pentoxide absorbs oxygen at a high temperature probably with the formation of a phosphate or pyrophosphate.

In the discussion of the results special reference is made to the work of Berliner and Berthelot, and it is pointed out that there is not sufficient evidence for the existence of such chemical compounds as  $\text{Pt}_{30}\text{H}_2$  and  $\text{Pt}_{30}\text{H}_4$ . Moreover, the authors are of opinion that the heats of combination of hydrogen and platinum as determined by Berthelot and Favre are valueless, and that the heat which they measured is due for the most part if not entirely to the formation of water by the oxygen always contained in platinum black. It has yet to be *proved* that the absorption of hydrogen by pure platinum black is attended by the evolution of heat, and as regards the formation of supposed true chemical compounds, solid solutions, or alloys, the authors prefer to wait until sufficient data have been accumulated for an adequate enquiry, before coming to any definite conclusion.

IX. "On the Electrolysis of Gases." By J. J. THOMSON, M.A.,  
F.R.S., Cavendish Professor of Experimental Physics,  
Cambridge. Received June 17, 1895.

In the experiments described in this paper I have used the spectro-scope to detect the decomposition of gases by the electric discharge and the movement of the ions in opposite directions along the discharge-tube.

The method consists in sending the electric discharge through a tube so arranged that the spectra close to the positive and negative electrodes can easily be compared; thus the presence or absence of certain ions at these electrodes can be ascertained. This method is capable of much wider application than the one I previously used in my experiments on the "Electrolysis of Steam" ('Proc. Roy. Soc.,' vol. 52, p. 90), the use of which is attended with very great difficulty for any substance other than steam. The earlier method has, however, the advantage of being a quantitative method—the present one is only qualitative.

In my former experiment with steam, when I worked at atmospheric pressure and varied the length of the spark, I found that when the spark-length exceeded a certain length,  $d_1$ , there was an excess of hydrogen at the negative electrode and of oxygen at the positive, equal in amount to the quantities of hydrogen and oxygen liberated from a water voltameter placed in series with the steam-tube. When the sparks were shorter than a certain length,  $d_2$ , the hydrogen appeared at the *positive*, the oxygen at the *negative* electrode, but the quantity of these gases was again equal to the quantities liberated in a water voltameter placed in series with the steam-tube.

When the spark-length was between  $d_1$  and  $d_2$  the effects were irregular, and there seemed to be no connection between the amounts of gases liberated in the steam-tubes and those liberated in the voltameter.

In the following experiments in which the sparks were of constant length and the pressure was altered, corresponding effects were observed. Within certain limits of pressure definite and perfectly regular evidence of the separation of the ions of the gas sparked through was obtained; and the electrode at which a given ion appeared could be reversed by altering the pressure; there was, however, a range of pressures in which the separation of the ions was either not well marked or was irregular in character.

I shall begin by describing a very simple method of showing the separation of the ions produced by the discharge of electricity through a compound gas such as hydrochloric acid gas, which is

applicable when the discharges through the constituent gases of the compound are of distinct and different colours; this is eminently the case with the hydrochloric acid gas, as the discharge through hydrogen in a capillary tube is red, through chlorine green.

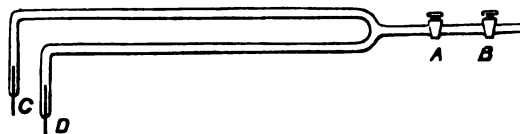
Take a capillary tube of very fine bore, the finer the better (the tube I used was thermometer tubing of the finest bore I could procure), and insert platinum wires for electrodes in two small bulbs blown on the ends of the tube; then fill the tube with HCl gas, allowing it to run through the tube for a considerable time so as to get rid of any extraneous gas, and exhaust the tube so that the gas in it is at a very low pressure. Then when the discharge from a large induction coil passes through the tube, the following phenomena are observed. When first the discharge passes through the tube the colour is uniform throughout and of a greenish-grey; after the discharge has been passing for a little time the end of the tube next the cathode gets distinctly red, whilst that next the anode gets green; this difference in the colour at the ends of the tube goes on increasing until the tube presents a most striking appearance, the part near the cathode being bright red, while that near the anode is a bright green. The difference in colour attains a maximum value, and if the discharge is allowed to run for several hours the contrast between the two ends disappears to a very great extent; the discharge throughout the whole of the tube being pinkish and apparently passing mainly through hydrogen. This is doubtless due to the diffusion through the tube of the hydrogen which in the earlier stages of the discharge had accumulated about the cathode; one advantage of using very narrow tubes is that with them this diffusion is slow. When the tube is in this condition the colour of the discharge sometimes changes suddenly, and for a second or two is green instead of pink, showing that though in the main the discharge passes through hydrogen, it occasionally leaves the hydrogen and passes through the chlorine. This transference of the discharge from one constituent to another of a mixture of gases is not infrequently observed when the gases are mixed in certain proportions.

Some of these capillary tubes showed after the discharge had been passing through them for some time a peculiar patchy appearance, some portions of the tube being a much brighter red than the others, while other portions were green. In some tubes this occurred to such an extent that the discharge showed an irregularly striated appearance. This effect is due, I believe, to gases or moisture condensed on the walls of the capillary tube, and in some cases to irregularities in the chemical composition of the glass. I found that it did not occur if the tube before being used was heated for some time along its whole length to as high a temperature as it would stand without collapsing; this heating would tend to cleanse the walls of the tube. That dif-

ferences in the quality of the gas also conspire to produce these patches is shown, I think, by the following phenomenon. A capillary tube of fine bore containing mercury vapour and a little water vapour developed a well marked red patch; the tube was then heated for some inches in the neighbourhood of the patch. In general heating the tube makes the discharge yellow from the sodium vapour given off from the glass; in this case, however, the whole of the heated portion, with the exception of the patch, turned yellow; the patch itself withstood the heating and continued to show the bright red colour characteristic of hydrogen.

*Electrolytic Transport of one Gas through another.*—A tube of the shape shown in fig. 1 was made of the finest bore thermometer-tubing; the extremities, C and D, of the tube in which the electrodes

FIG. 1.



were fused were bent down so as to be parallel to each other, and so near together that a slight motion of the tube suffices to bring either of the extremities in front of the slit of the spectroscope. The tube was mounted on a board moved by a lever; by moving this the observer at the spectroscope could readily bring the spectrum of either the positive or negative electrode into the field of view. A side tube, AB, was fused to the middle of the main tube and was provided with two taps; in the space between these taps a small quantity of any gas which it was desired to introduce into the main tube could be imprisoned, and could, by opening the tap A, be introduced into the discharge tube. The experiment consists in filling the main tube with a gas at a low pressure, observing the spectra at the two electrodes, then introducing by the side tube a very small quantity of gas into the main tube, and again observing the spectra at the two electrodes.

A tube was filled with hydrogen and showed no trace of the chlorine spectra; a very small quantity of chlorine was then let in through the side tube (in performing this experiment it is necessary to be careful that only a very small quantity of chlorine is introduced). After the discharge had been running through the tube for a short time, the chlorine spectrum was found to be bright at the positive electrode, though no trace of it could be detected at the negative. When the discharge was kept on for some time, the chlorine spectrum, though still visible at the positive electrode, got fainter; it did not appear at all at the negative. If a considerable

quantity of chlorine was introduced through the side tube, the chlorine spectrum was visible at both electrodes, though it was brighter at the positive than at the negative.

When the induction coil was reversed, so that what was before the positive electrode became the negative, the first effect observed was that the chlorine spectrum flashed out with great brilliancy at the old positive electrode, and was much brighter than at any previous period. This, however, only lasted for a second or two; the chlorine spectrum rapidly faded away and for a time was not visible at either electrode. Soon, however, the chlorine spectrum appeared at the new positive electrode, having thus been transferred from one end of the tube to the other.

On again reversing the coil the same phenomenon was repeated. There was apparently no limit to the number of times this effect may be obtained; at any rate, I have driven the chlorine from one end of a tube to the other 14 times in succession by reversing the coil. The chlorine is always driven to the positive electrode, showing that the chlorine ion carries a charge of negative electricity. The same effect was obtained when a little vapour of bromine was introduced into the tube instead of chlorine. When, however, the capillary tube was filled with chlorine instead of hydrogen, and a little vapour of bromine let into the tube, the bromine went to the *negative electrode* instead of to the positive, as it did when introduced into the hydrogen tube. These experiments suggest that the two gases in the tube combine, and that the compound gas so formed is split up into ions which travel along the tube; that bromine when in combination with hydrogen is the negative ion, and therefore travels to the positive electrode; when, however, it is in combination with chlorine the bromine is the positive ion and travels to the negative electrode.

Another experiment tried was to let a little vapour of sodium into the middle of a capillary tube filled with air at a low pressure. To prevent the sodium vapour condensing on the walls of the tube, the whole tube was placed on a sand-bath and the temperature raised so high that no condensation took place. After the discharge had run through the tube for about two hours the sand was removed from the tube, and the movement of the sodium vapour to the *negative electrode* was very apparent even without using a spectroscope, as there was a great patch of yellow light near the negative electrode and none in any other part of the tube.

Another experiment was to introduce a small quantity of hydrogen into a tube filled with air at a low pressure: the hydrogen made its way to the *negative electrode*. This experiment is a somewhat troublesome one, as it is exceedingly difficult to get these very fine capillary tubes so dry that the spectrum of the discharge does not show the hydrogen lines even before the hydrogen is introduced into the middle

of the tube; indeed I never succeeded in getting rid of the hydrogen lines at the very lowest pressures. By heating the tube and allowing dry air to run through it for a long time, however, I got the tube so dry that it did not show the hydrogen lines at a pressure quite low enough to allow the discharge to pass freely through it. When the tube was in this state and hydrogen was let into the middle of the tube, the hydrogen spectrum appeared at the negative electrode, but not at the positive.

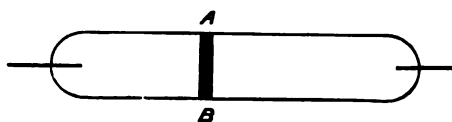
The appearance of hydrogen at the negative electrode when mixed in a discharge tube with other gases has been described by Mr. Baly in a very interesting paper in the 'Philosophical Magazine,' vol. 35, p. 200.

The preceding experiments suggest, I think, that this separation of two gases, A and B, by the discharge is due to the decomposition by the discharge of a chemical compound formed of A and B, in which the A atoms have a charge of electricity of one sign, the B atoms a charge of electricity of the opposite sign; these charged atoms under the influence of the electromotive force in the tube travel in opposite directions. Further, it follows from the experiment with the bromine vapour in an atmosphere of chlorine that the sign of the electrical charge on an atom of the same substance is not invariable, but depends on the substance with which this atom is in combination. We shall find numerous other instances of this change in the sign of the charge on an atom in experiments described in a later part of this paper.

*Polarisation of the Electrodes.*—This in the electrolysis of liquids is due to the accumulation at the electrodes of ions which have ceased to act as carriers of electricity. We have, I think, distinct evidence of a similar accumulation in the electrolysis of gases. For, as has been already described, after the discharge has been running for some time in one direction, giving the spectrum of some gas at one of the terminals, the spectrum of the gas at that terminal is momentarily brightened to a very great extent by suddenly reversing the direction of the discharge. After the current has been flowing for some time in one direction through, say, Cl in an atmosphere of H, the spectrum of the chlorine, though still visible at the positive electrode, gets faint, the chlorine apparently to a great extent ceasing to carry the discharge; when, however, the current is reversed, the atoms of chlorine can move freely, as they are not obstructed by the electrode, so that immediately after the reversal of the current there is probably more of the discharge carried by the chlorine than at any other time, and the chlorine spectrum is consequently brightest.

*Discharge through a Compound Gas.*—The separation of the ions by the discharge can be readily observed in a tube of the kind shown in fig. 2.

FIG. 2.



It differs from an ordinary discharge tube merely in having a flat metal plate, AB, fastened across the tube. When the discharge passes through the tube, one side of the plate acts as a positive, the other as a negative, electrode. The tube is mounted on a stand, which the observer at the spectroscope can move by means of a lever so as to bring one side or other of the plate opposite the slit of the spectroscope; a very slight movement of the lever is sufficient to do this, so that the spectra at the two sides of the plate can readily be compared. I found that the results were more satisfactory when the current was kept flowing through the tube in one direction and the tube moved so as to bring the spectra at the two electrodes into the field of view, than when the tube was kept fixed in one position and the current reversed. The latter method, however, suffices to show the separation of the ions in many cases, and it has the advantage of not requiring a plate across the tube; all that is necessary is to use for one of the terminals a disc whose plane is parallel to the slit of the spectroscope.

If the plate AB is thin, it is necessary to fuse it into the glass tube all the way round, otherwise when the pressure is low, the discharge, instead of crossing the plate, goes through any little crevices there may be between the plate and the tube. The easiest way of making the tube is to use a plate about 0.5 cm. thick, cut from an aluminium cylinder which tightly fits the tube; with a plate of this thickness the narrow spaces between the tube and the plate are so long that the discharge goes through the plate rather than through the crevices.

The tube was filled with the gas to be observed and the spectra at the two sides of the plate compared. These spectra were in many cases found to differ in a very remarkable way; it was, however, only in exceptional cases that a line which was bright at one side of the plate was absolutely invisible on the other. The method used was to take two sets of lines, say A and B, as close together in the spectrum as possible, and compare the brightness of these sets of lines on the two sides of the plate; if it was found that the A lines were brighter on the positive side of the plate than on the negative, while on the other hand the B lines were brighter on the negative side of the plate than on the positive, then it was inferred that electrolytic separation had occurred, and that the substance giving the A lines was in excess on the positive side of the plate, that giving the B lines on the nega-

tive. It is not safe to draw any conclusions from the variations in intensity of one line or one group of lines on the two sides of the plate, as the total quantity of light coming from the neighbourhood of the cathode often differs considerably from that coming from the anode. When, however, we get an increase in the brilliancy of one set of lines accompanied by a diminution in the brightness of another set, when we move across the plate we eliminate this source of error. The differences in the spectra at the two sides of the plate are most easily observed at pressures where there is not any very great difference between the luminosity of the cathode and the anode. As was mentioned at the beginning of the paper, there is a range of pressure within which the effects are irregular, and no decided differences are observed between the spectra at the two sides of the plate. It is desirable in these experiments to keep the tube on to the pump as long as the experiment lasts, for the discharge always decomposes the compound gas, and unless the products of decomposition are continually pumped off and replaced by fresh supplies of the compound gas, the spectra of the discharge keep changing. With organic compounds this is especially necessary, as the character of the spectrum often changes entirely very shortly after the commencement of the discharge unless fresh gas is continually introduced.

In the following experiments the current was produced by a large induction coil with a mercury slow break.

When the tube was filled with hydrochloric acid gas at a low pressure, the separation of the hydrogen and chlorine was seen very distinctly, the hydrogen line being much brighter on the side of the plate which acted as the cathode (which we shall call the negative side of the plate) than on the positive side, while the chlorine, on the other hand, was brighter on the positive than on the negative side of the plate.

When the tube was filled with ammonia gas, the hydrogen lines were bright on the negative side of the plate but were absent from the positive side, while on the positive side of the plate there was the positive pole spectrum of nitrogen, and on the negative side of the plate the negative pole spectrum of nitrogen and the hydrogen spectrum.

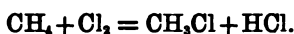
*Sulphur Monochloride.*—When the tube was filled with the vapour of this substance at a low pressure, the chlorine lines were brighter on the negative side of the plate than at the positive, while the sulphur lines were brighter at the positive side than at the negative. Thus the chlorine in this substance behaves in the opposite way to the chlorine in  $\text{HCl}$ ; in the latter compound the chlorine ion has a charge of negative electricity, while in the sulphur monochloride it has a charge of positive electricity.

*Influence of the Chemical Constitution of a Compound on the Sign of the Charge of Electricity on one of its Constituent Atoms.*—In many organic compounds an atom of the electro-positive element hydrogen can be replaced by an atom of the electro-negative element chlorine without altering the type of the compound. Thus, for example, we can replace the four hydrogen atoms in  $\text{CH}_4$  by chlorine atoms, getting successively the compound  $\text{CH}_3\text{Cl}$ ,  $\text{CH}_2\text{Cl}_2$ ,  $\text{CHCl}_3$ , and  $\text{CCl}_4$ . It seemed of interest to investigate what was the sign of the change of electricity on the chlorine atom in these compounds. The point is of some historical interest, as the possibility of substituting an electro-negative element in a compound for an electro-positive one was one of the chief objections assigned against the electro-chemical theory of Berzelius.

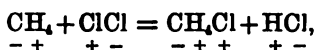
When the vapour of chloroform,  $\text{CHCl}_3$ , was placed in the tube, it was found that both the hydrogen and the chlorine lines were bright on the negative side of the plate, while they were absent from the positive side, and that any increase in the brightness of the hydrogen lines was accompanied by an increase in the brightness of those due to chlorine. The spectrum on the positive side of the plate was that called the carbonic oxide spectrum; when first the discharge passed through the tube, the spectrum on the positive side was the so-called candle spectrum, but this very rapidly changed to the carbonic oxide spectrum. The appearance of the hydrogen and chlorine spectra at the same side of the plate was also observed in methylene chloride and in ethylene chloride. Even when all the hydrogen in  $\text{CH}_4$  was replaced by chlorine, as in carbon tetrachloride,  $\text{CCl}_4$ , the chlorine spectra still clung to the *negative* side of the plate. To test the point still further, I tried the analogous compound silicon tetrachloride, inserting a small jar in the circuit to brighten the spectrum. The chlorine spectrum was again brightest at the *negative* side of the plate, while the silicon spectrum was brightest at the positive. This is a very favourable case for the application of this method, as there are two silicon lines (wave-lengths 5058, 5043) quite close to two chlorine ones (wave-lengths 5102, 5078), so that their relative brightness can easily be compared. The experiment with the silicon tetrachloride is more conclusive than those with the carbon compounds, as with the latter the spectrum on the positive side of the plate is a band spectrum, and since the potential gradient when the discharge is passing is very much steeper on the negative side of the plate than on the positive, the effects observed might be supposed to be due to the circumstances on the negative side being better adapted for the production of line spectra than those on the positive. This explanation is not, however, applicable to the case of silicon tetrachloride, where the spectra on both sides of the plate are line spectra.

From these experiments it would appear that the chlorine atoms in the chlorine derivatives of methane are charged with electricity of the same sign as the hydrogen atoms they displace.

When we can determine the signs of the electrical charges carried by the atoms in a molecule of a compound, we can ascertain whether any given chemical reaction does or does not imply interchange between the electric charges on the atoms taking part in the reaction. Thus take the reaction

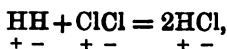


If we represent the sign of the charge of electricity carried by an atom by + or - placed below the symbol representing that atom, we may write the last reaction as



so that this reaction could be produced by a rearrangement of the atoms without any alterations of their electrical charges.

If, however, we take the reaction—



we see that in addition to a rearrangement of the atoms there must in this case be an interchange of electric charges between the atoms; for before combination half the hydrogen atoms had a negative charge, and half the chlorine atoms a positive one, whereas after combination no hydrogen atom has a negative charge, and no chlorine atom a positive one. We may thus distinguish between two classes of chemical reactions, (1) those which do not necessarily require any interchange of the electrical charges carried by the atom, and (2) those which do. It might, perhaps, repay investigation to see whether the occurrence of chemical change is affected by the presence of a third substance in the same way in these classes of chemical combination.

Another point to be considered is the effect of this difference between the chemical actions on the amount of heat developed during chemical combination. When hydrogen and chlorine combine the heat produced may be regarded as the joint effect of three processes:—

1. The splitting up of the molecules  $\begin{array}{cc} \text{H} & \text{H} \\ + & - \end{array}$  and  $\begin{array}{cc} \text{Cl} & \text{Cl} \\ + & - \end{array}$  into the atoms  $\begin{array}{cccc} \text{H} & \text{H} & \text{Cl} & \text{Cl} \\ + & - & + & - \end{array}$ .

2. A transference of electricity by which the negative charge on one atom of hydrogen is replaced by an equal positive charge, while the positive charge on an atom of chlorine is replaced by an equal negative charge.

3. The combination of the positively electrified hydrogen atoms with the negatively electrified chlorine ones to form hydrochloric acid.

In that class of chemical action where the atoms retain their charge (2) is absent, so that if the change in energy occurring in the process (2) were considerable compared with the changes occurring in processes (1) and (3), the thermal effects of the two types of chemical combination ought to differ considerably. If the changes in energy occurring in the process (2) had a great preponderance over those occurring in (1) and (3), the thermal effects produced by the combination of two elements ought to follow very simple laws. For if  $2\{H\}$  is the excess of the energy of an atom of hydrogen charged with the negative electron over the energy of the atom charged with the positive electron,  $2\{Cl\}$  the excess of the energy of an atom of chlorine charged with the positive electron over the energy of the atom charged with the negative electron, then if we could neglect the energy changes in (1) and (3) compared with those in (2), the mechanical equivalent of the heat developed when a molecule of hydrogen combines with one of chlorine to form two molecules of hydrochloric acid would be equal to  $2\{H\} + 2\{Cl\}$ . Thus we see that if the energy changes in (2) preponderated largely over those in (1) and (3), the heat produced when an element A combined with another element B to form the compound AB, could be expressed as the sum of two numbers  $\{A\}$  and  $\{B\}$ , where  $\{A\}$  depends solely on the element A,  $\{B\}$  solely on the element B. In some cases of chemical combination between dilute solutions there seems evidence that the heat produced can be expressed in this way (see Lothar Meyer, "The Evolution of the Doctrine of Affinity," 'Phil. Mag.,' vol. 23, p. 504), but when we attempt to apply the same law to combination between gases, it seems utterly to break down, indicating that in such cases the greater part of the changes in energy occur in the splitting up of one set of molecules and the subsequent formation of others. This view seems to be supported by the phenomena attending the discharge of electricity through rarefied gases, for the smallest difference of potential which can send a discharge through an electrified gas (which we have reason to believe involves the splitting up of molecules into atoms), is very many times the electromotive force required to liberate the ions from an electrolyte, though the latter process requires changes in the electrical charges on the ions. These reasons seem to indicate that we can hardly expect to get any clear indication of the charges carried by the atoms in gaseous compounds from the study of the thermal changes which occur when gases enter into chemical combination.

*Vapours of Organic Compounds.*—These show very interesting differences between the spectra on the two sides of the plate when

the discharge passes through them. Thus when the discharge first passes through the vapour of ethyl alcohol,  $C_2H_5O$ , the spectrum on the positive side of the plate is the candle spectrum, that on the negative side the carbonic oxide spectrum. For some little time after the discharge commenced I could not detect any hydrogen lines on either side of the plate; after a time, however, they appeared on the negative side but not on the positive. If the discharge was kept running for some time without letting a fresh supply of alcohol into the tube, the "candle spectrum" on the positive side of the plate was replaced by the CO spectrum, which now occurred on both sides of the plate, accompanied on the negative side by the hydrogen spectrum. This is the appearance presented by all the compounds of carbon, oxygen, and hydrogen which I examined, when the spark had been passing through them for a considerable time, and it is what would occur if the vapour were decomposed by the spark into carbonic acid, water, and hydrogen.

The appearance of the candle spectrum on the positive side of the plate with the CO on the negative was observed in many other cases. Thus on sparking through a tube filled with CO I could not detect any difference between the spectra on the two sides of the plate, but when a little hydrogen was let into the tube the "candle spectrum" appeared on the positive side of the plate, the carbonic oxide spectrum on the negative. The same effect was observed in a tube filled with cyanogen mixed with a little hydrogen. When the tube was filled with the vapour of methyl alcohol,  $CH_3OH$ , the candle spectrum was on the positive side of the plate, the carbonic oxide and hydrogen spectra on the negative; with this vapour, unlike that of ethyl alcohol, I could not detect any stage when the hydrogen spectrum was absent.

The first explanation which occurs to one of this phenomenon is that it is owing to the potential gradient at the negative side of the plate being steeper than that on the positive, so that we may imagine we have a fierce spark on the negative side, a mild one on the positive, and that the fierce spark gives the CO spectrum, the mild one the candle spectrum. There are, however, some phenomena which seem inconsistent with this explanation: in the first place if the current is reversed after flowing in one direction, traces of the former spectra linger for some time at the sides of the plates, and, secondly, if the difference is due to the greater decomposition at the negative side of the plate, how is it that in the case of the vapour of ethyl alcohol the hydrogen spectrum is not seen, at the commencement of the discharge, on the negative side of the plate? It only appears after the discharge has passed through for some time, when hydrogen has probably been set free by the decomposition of the vapour by the discharge. If the absence of the candle spectrum from the negative side

of the plate is due to the spark being so intense that the hydro-carbon which is supposed to be the origin of this spectrum cannot exist, then we ought to see the spectra of the substances which result from the decomposition of the hydro-carbon, i.e., we ought to see the hydrogen spectrum at the negative electrode. The view which seems most in accordance with the results of observations on the discharge through these vapours is that the "candle spectrum" is the spectrum of carbon when the atom is charged with negative electricity, or of some compound of carbon in which its atom is negatively charged, while the "carbonic oxide" spectrum is the spectrum of carbon when the atom is charged with positive electricity, or of some compound in which the carbon atom is positively charged.

*Discharge through an Elementary Gas.*—It has long been known that when the discharge passes through some elementary gases, the spectra at the two electrodes are different. This was first shown to be the case for nitrogen, then Dr. Schuster showed that the same thing occurred with oxygen, and recently Mr. Crookes has shown that it is also true in the case of argon. I have observed a very striking change in the relative brilliancy of the red and green hydrogen lines at the two electrodes. When the tube with the plate across it was filled with hydrogen at a low pressure, then on the positive side of the plate the red line tends to be brighter than the green, while on the negative side the green line tends to be brighter than the red; in some tubes this was so marked that on the positive side of the plate the red line was bright, and the green invisible, while on the negative side of the plate the green line was bright, and the red invisible. The spectroscope I was using weakened the red rays much more than the green, so that I cannot be sure that the red rays were really altogether obliterated on the negative side of the plate; the above experiment is, however, sufficient to show that on the positive side of the plate the red rays are more easily excited than the green, while on the negative side the green line is more easily excited than the red. On the negative side of the plate we have an excess of positively charged hydrogen atoms, while on the positive side of the plate there is an excess of negatively charged hydrogen atoms, and I am inclined to attribute the difference in the spectra partly at any rate to the difference in properties between a positively and a negatively charged hydrogen atom. The reason I do not attribute it wholly to the difference in the potential gradient on the two sides of the plate is that the effect is not reversed immediately, but only gradually on reversing the coil, the former spectra clinging for some time to the sides of the plate.

*Chlorine.*—I have made a great many experiments to see if there is any difference between the spectra given by chlorine on the two

sides of the plate, but with negative results. Chlorine seems a gas in which we might expect to find this effect, for as Dr. Schuster, in his Report on Spectrum Analysis, says, the behaviour of its spectrum indicates that we have several spectra superposed. I have not, however, been able to effect a separation of its spectra, the differences I observed between the spectra on the two sides of the plate were irregular, and due, I think, to impurities producing effects like those observed when the discharge passes through a compound gas. However, as has been mentioned before, there is even in the case of gases where distinct evidence of separation can be obtained, a region of pressure within which the effects are irregular, and I ascribe my failure to observe separation in the case of chlorine to my having failed to get the relation between the intensity of the discharge and the pressure so adjusted as to get outside this irregular region. The cases, however, in which distinct differences between the spectra of a simple gas occur at the two electrodes, seem to indicate that the spectrum given by an element is influenced by the sign of the electrical charge carried by its atoms.

I have made some experiments to determine whether there was any separation produced in a mixture of equal volumes of hydrogen and chlorine kept in the dark, when a considerable difference of potential though not sufficient to produce discharge was maintained between the two electrodes. The parts of the tube adjacent to the two electrodes could be shut off from each other by a tap, and the amount of chlorine in the two sides was determined by absorbing it by caustic potash. The mixture was at atmospheric pressure, and the electrodes were maintained at a potential difference of about 1,200 volts by connecting them to a large battery of small storage cells. The potential difference between the terminals was maintained for about 16 hours on three separate occasions, but on analysing the vessels surrounding the two electrodes, the amount of chlorine in the vessel adjacent to the negative electrode did not differ from that in the vessel adjacent to the positive electrode by more than 1 per cent., and this could be accounted for by errors of experiments, as test experiments in which the mixture had not been exposed to the electric field gave differences comparable with these. We should conclude from the preceding experiments that the molecules of a gas are not acted on by any appreciable translational force tending to move them from one place to another, when they are near to a body charged with electricity. To test this point further, two large terminals were placed in bulbs which were connected by a horizontal capillary tube, in which a drop of sulphuric acid was placed; a difference in the pressure of the gas would cause the sulphuric acid to move, and the arrangement acts as a very delicate pressure gauge. The bulbs and tube were filled with

chlorine at atmospheric pressure. The terminals were then connected to the electrodes of a battery giving a potential difference of 1,200 volts, but not the slightest movement of the drop of acid could be detected.

I wish to acknowledge the help I have received in making the preceding experiment from my assistant, Mr. E. Everett.

X. "On the Evolution of the Vertebral Column of Amphibia and Amniota." By HANS GADOW, M.A., Ph.D., F.R.S.  
Received June 20, 1895.

(Abstract.)

The key to the solution of the composition of the vertebral column is given by the metameric repetition of the four pairs of symmetrically arranged cartilaginous elements, the origin of which we have traced in Fishes, namely—

One pair of basidorsalia.

One pair of basiventralia (with its lateral outgrowths = ribs or pleurapophyses, and hæmal, ventral outgrowths = hæmal arches, chevrons, wedge-bones, hæmapophyses).

One pair of interdorsalia.

One pair of interventralia.

The first of these four pairs is always present and forms the neural arch. Of the other three pairs any one may be suppressed, sometimes even two in the same skleromere.

The vertebræ of the Amphibia and Amniota possess only arch-centra, because the chordal sheath takes no share in their formation.

According to the composition of these arco-genous centra or bodies we distinguish the following lines of evolution :—

I. Earliest primitive condition. All the four pairs of arcualia are present :—

1. Either as separate pieces, of which the interventralia are the smallest, e.g., *Archegosaurus*, *Chelydosaurus*, *Larvæ of Urodela*.
2. Or the basidorsalia and basiventralia fuse together and form the middle of the vertebral body, while the interbasalia form neutral zones of intervertebral cartilage. Such PSEUDOCENTRA are those in the tail of *Urodela*.

II. The interventralia are reduced.

1. The other three pairs remain separate, and the interdorsalia tend to form the centrum. (NOTOCENTROUS

type, e.g., trunk of *Euchirosaurus*, *Actinodon*, *Trinero-rhachis*.)

2. The three pairs of basidorsalia, interdorsalia, and basiventralia co-ossify. (Trunk vertebræ of most recent *Amphibia*, caudal vertebræ (coccyx) of *Anura*.)

III. Interventralia and basiventralia are reduced. The vertebræ consist of dorsal elements only, so-called "epichordal" type. (Trunk of *Pipa*, *Xenopus*, *Bombinator*.)

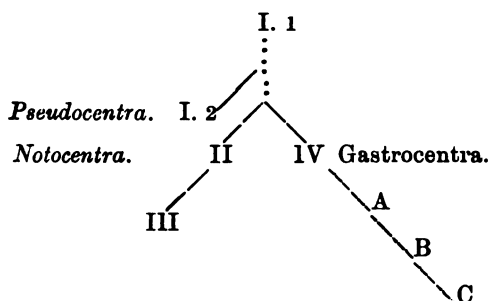
IV. The interdorsalia are reduced. The interventralia are enlarged and form the centrum. (GASTROCENTRUS type.)

IVa. The three remaining components ossify separately and remain separate; the basiventralia are still large and carry most of the neural arch. (All the vertebræ of *Eryopse*, *Cricotus*, the atlas of the *Amniota*.)

IVb. The centrum is much enlarged, forms the whole of the "body," and carries the neural arch (neurocentral suture). The basiventralia are much reduced and form the "intervertebral disks" or menisci, attached to the cranial end of the centrum; when less reduced, they appear as the so-called wedge bones, "intercentra," or chevrons. (Trunk and tail of most *Amniota*.)

IVc. The centrum and the neural arches alone constitute the vertebra. The basiventrals are lost completely. (Trunk of many *Amniota*, for instance, the thoracic and lumbar vertebræ of lizards, caudals of many mammalia and birds.)

The phylogeny of these modifications may be expressed as follows:—



The *ribs* are homologous structures throughout the vertebrata. They are lateral distal outgrowths of the original basiventralia, with which they later on form joints, and they ossify independently.

The "chevrons," the bony arms or arches which enclose the caudal canal, are more ventral or median outgrowths of the same basiventralia and are to be called hæmapophyses. They are possibly serially

homologous with the visceral arches, but any attempt to homologize them with, or to consider them as distal outgrowths of the inter-ventralia, results in failure. Ribs and hæmapophyses are not homologous structures, although both are ventral arches in a general sense.

The ribs are originally attached by their capitula, to the basi-ventralia; the tubercular attachment to the neural arch is a secondary acquisition. When the basi-ventralia are reduced, the ribs *either* retain their old attachment and appear henceforth as "intervertebral" organs, *e.g.*, in many mammalia, in the anterior cervicals of Hatteria, and in many vertebræ of Chelonia; *or* they transfer their attachment backwards upon the centrum, in some cases even upon the neural arches.

The *spinal nerves* arise originally in a transverse level behind the basidorsalia and in front of the interdorsalia, *i.e.*, intravertebrally. Each nerve issues behind, or through the neural arch of the vertebra to which it genetically belongs. The first spinal nerve, when it is a N. sub-occipitalis (although this nerve is not in all vertebrata the same serial entity) has lost its vertebra, the latter being added to the skull, either completely, without leaving post-cranial remnants (*Amniota*), or incompletely, and in this case the ventral arcualia, probably the inter-ventralia only, are added to the first vertebra as an odontoid-like process (*Urodela*).

- XI. "Spectra from Swedish Bessemer Works." By C. J. LUNDSTRÖM. Communicated by Professor HARTLEY, F.R.S. Received April 1, 1895.

[Publication deferred.]

- XII. "Remarks on the Origin of some of the Lines and Bands observed in the Spectra from Swedish Bessemer Works." By W. N. HARTLEY, F.R.S., Professor of Chemistry, Royal College of Science for Ireland. Received April 1, 1895.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 21st.

*Presents, June 20, 1895.***Transactions.**

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*Fourth Report to the Royal Society Water Research Committee.*

By PERCY F. FRANKLAND, Ph.D., B.Sc., F.R.S., Professor of Chemistry in Mason College, Birmingham, and H. MARSHALL WARD, D.Sc., F.R.S., F.L.S., F.R.H.S., Professor of Botany, Royal Indian Engineering College, Cooper's Hill. Presented to the President and Council, March 14, 1895.

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"On the Biology of *Bacillus ramosus* (Fraenkel), a Schizomycete of the River Thames." By H. MARSHALL WARD, D.Sc., F.R.S., F.L.S., F.R.H.S., Professor of Botany, Cooper's Hill.

INTRODUCTORY.

*Morphology and Classification.*

During the progress of my investigation of the bacterial flora of the River Thames, I have frequently isolated from the water a schizomycete, which turns out to be in many respects one of the most interesting forms yet met with, and probably one of the most instructive species yet known to science.

It occurs at all seasons, more or less, but oftenest during the autumn and winter months, and is found on the isolation plates as white colonies, becoming yellowish with age, which rapidly develop into large membranous growths, so like the mycelium of a fine mould that it is quite conceivable, or even probable in some cases, observers might pass it by as a small mucor or other fungus, not belonging to the schizomycetes at all.

It appears on the plates at all times of the year, though it seems to be commoner in the river water taken in autumn and winter than in the samples of spring and summer.

It attracted my attention at an early period in the investigation, owing to its rapid growth, the relatively large size of the filaments and cells, and especially the facility with which it can be made to develop spores. Moreover, it presents certain superficial, though not unimportant, resemblances in some of its characters to cultures of *Bacillus anthracis*, from which species, however, it is decidedly distinct, as evinced by its larger size and other pronounced characters.

I have now, after long and careful studies of its behaviour on different media, and in various circumstances, little hesitation in referring this species to Fraenkel's *Bacillus ramosus*, the form commonly known as the *Wurzel-bacillus* in Germany, but which passes under various names, and concerning which there is still a good deal of confusion in the various text-books and bacterial floras which mention it.

I shall discuss these matters and the synonymy at the end of the present section of the paper; but since I have succeeded in following out the life-history of this species in a singularly complete manner, and find the organism a remarkably typical and instructive one, it has seemed worth while to give in detail all the facts which have come under my observation, and especially to call the reader's attention to the fact that it runs through its entire life-history, from spore to spore-formation, in from thirty to sixty hours at ordinary temperatures,\* and that I have been enabled to follow the course of this life-history by continuous observations under powers ( $1/12$ th and  $1/20$ th oil-immersions) much higher than have commonly been successfully employed for such observations.

#### *Bacteriological Cultures.*

In the preliminary plate-cultures in 10 per cent. gelatine, made to isolate it from the Thames water, the colonies look so like the mycelium of a fine white mould fungus, that, as said, it is quite conceivable they might be overlooked or neglected by observers not sufficiently on their guard concerning such deceptive forms. In fig. 1 is represented one of these colonies, natural size, as seen on the second day, at  $15^{\circ}$  C. From a darker centre, which already shows signs of softening and liquefying the gelatine, wavy strands, of varying diameters, radiate outwards, and break up into finer and finer strands or filaments, until they fade away imperceptibly at the margins. The whole of this circular colony has a milky-white, somewhat opaque appearance, especially in the denser centre, which may have a yellowish tinge now or a little later; as we approach the indistinct margins, the opacity gradually gives way to a translucency, and eventually transparency, which prevent sharp demarcation from the gelatine around.

In a recorded series of observations of such colonies, at  $15^{\circ}$  C., I found they were first visible to the unaided eye in about forty to forty-eight hours; in seventy-two hours the average diameter was 25 mm.; in ninety-six hours about 50 mm.; and in six days (168 hours) a single colony covered nearly half the area of a Petri dish.

\* The ease with which it can be obtained and grown suggests that this species would be a much better type for teaching purposes than the smaller *B. subtilis* commonly used.

Examined under a low power ( $\frac{1}{3}$ -in. objective), the radiating strands are found to consist of closely woven and interwoven, wavy, and coiled tresses of filaments, with a distinct yellowish colour such as protoplasmic structures usually present by transmitted light. These tresses (fig. 2) break up into finer and finer strands, as said, towards the margin, and finally into single filaments.

The coarser tresses may consist of twenty to thirty closely twisted or woven filaments, and are perfectly evident even to the unaided eye in older (*e.g.*, six days) colonies, the margins of which, as shown in fig. 3, remind one of a complex river-system as depicted on a map, or even of the yellowish, radiately-veined plasmodium of some myxomycete.

I suppose this radiating meshwork of yellowish-white strands was one character that suggested the resemblance to a network of fine rootlets, creeping on a flat substratum, and so gave origin to the German name (*Wurzel-bacillus*) for this species.

As we shall see subsequently, the individual filaments, twisted and woven to form the tresses and meshwork referred to, are the very long, wavy, coiled, or straight cell-series of the schizomycete.

The liquefaction of the gelatine at 15° C. commences only slowly about the second day, when the centre of the colony is soft beneath the dense membrane of tresses; it gradually progresses day by day, until, about the fourth day, the plate can no longer be upturned without liquid oozing from below. In about a week most of the colonies are floating on the fluid mass of liquefied gelatine. The liquid is slimy, and never seems to become completely watery, as is the case with some species. At higher temperatures, such as 20° and 25° C., the liquefaction is more rapid, in proportion to the quicker development and growth generally.

Colonies submerged deep down in the solid gelatine of a tube grow also, though more slowly than those at the surface; this is enough to show that the organism is partially anaërobic, though only to a very slight extent. The stab-cultures to be described demonstrate the same fact. The ultimate filaments themselves are long, straight, wavy, or spirally-coiled cylinders, averaging about 1.75  $\mu$  in diameter, and regularly divided into segments by transverse septa, at intervals of from 3 to 5 or 6  $\mu$ . It depends on the stage of development and other factors how far these segments are isolated from one another. In some cases, especially in the young colonies, the cylindrical filaments are merely septate; in others they are indented at the septa, and we may then speak of the filaments as more properly segmented; while in yet other cases the segments are so nearly isolated—evidently by more or less complete fission of the septa—that it is almost necessary to regard the filaments as long chains of segments end to end.



*Fig. 1.*



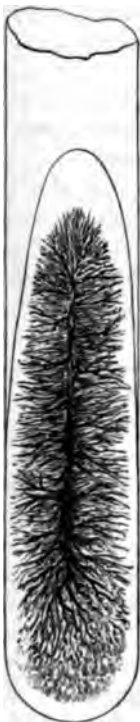
*Fig. 2.*



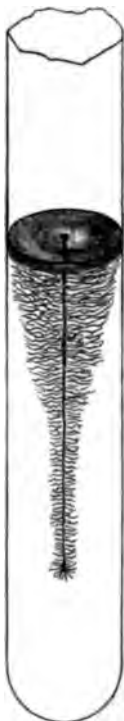
*Fig. 3.*



*Fig. 6.*



*Fig. 4.*



*Fig. 5.*



*Fig. 7.*



*Fig. 8.*

It hardly needs pointing out that these are merely so many stages in the progressive segmentation into *bacilli*, each bacillus being therefore a cylindrical rodlet, from 3 to 5 or 6  $\mu$  long by about 1.75  $\mu$  broad, and with rounded ends (see figs. 9—13).

Before this complete segmentation into bacilli, however, which only occurs, as a rule, very late in the life-history, the septate filaments often break across here and there at a septum, and so we have shorter filaments, each with twenty, fifty, or a hundred or more septa.

As I shall have to recur to these matters when tracing the development, however, no more need be said here further than that filaments and rodlets alike are usually quiescent, even in the liquefied gelatine,\* and no cilia can be detected at any time; for the very slow growth-movements to be described later would hardly come under the head of motility in the sense ordinarily used in bacteriology.

Streak-cultures on gelatine at 15° C. result in very pretty and characteristic growths. A rapid extension of the tresses and filaments above described is effected from the streak all over the surface of the gelatine, so that in three or four days the white mycelium-like membrane covers nearly the whole area (fig. 4), and presents some resemblance to the diagram of the digestive and blood systems of certain worms. Meanwhile the gelatine begins to soften and liquefy, and on the fifth day the film, more or less broken up, is floating on the liquid. In ten days nearly all the gelatine is liquefied, and flocks of broken film float on and in it. The liquid does not show any noticeable turbidity at any time.

Stab-cultures in 10 per cent. gelatine at 15° C. are even more beautiful. From the white axis formed along the line of puncture, radiating silky-looking filaments branch out horizontally towards the walls of the tubes, and by the second or third day the culture looks so like a root, with its silky root-hairs radiating around, that this stage would seem best to justify the German name *Wurzelbacillus*: it resembles strongly some cultures of *Bacillus anthracis*. On the fourth day liquefaction of the gelatine has definitely begun at the top, and the "root-hair system" now looks more like an inverted fir-tree (to use Fraenkel's simile), and this resemblance becomes more and more perfect as the culture gets older (see figs. 5, 6, and 7).

The liquefaction of the gelatine progresses slowly from above downwards, and the felted yellowish-white membrane floating on the top gradually breaks into flocculent patches. Even after three weeks the liquefaction has only extended about one-eighth of the distance from the top of the gelatine, but the fir-tree resemblance is

\* A slow movement of isolated segments breaking off at high temperatures (about 35° C.) must be regarded as abnormal, as will be seen later on.

more pronounced on inverting the culture, owing to the depression of the longer lower branches (fig. 7).

At 20° C. the development is similar, but the "root-hair" or "inverted fir-tree" system and liquefaction proceed more rapidly.

Cultivated as a streak on agar at 20° C. the development is somewhat like that of the streak-cultures on gelatine, except that the widening streak and fimbriated offshoots which it gives off at right angles (fig. 8) are of a more opaque, granulated, chalky-white appearance, presenting resemblances to cotton-wool rather than to a silk texture. About the third day this granulated, chalky, mould-like growth has begun to develop spores in abundance, and as these ripen and the filaments break up into segments, the whole assumes a white pasty consistency.

Cultivated on potato at 20° C., this schizomycete grows rapidly at first, and in twenty-four hours looks like a dry, white mould spreading over the surface. After about forty-eight hours the surface of the thickish membrane formed becomes still dryer and duller in appearance, and by the third day the mass looks like a slightly wrinkled, powdery, rather thick, drying up pasty layer, white tinged with yellow or grey. In this stage further extension ceases and spores are forming, and after five or six days magnificent crops of well-ripened spores are to be obtained from these potato-cultures.

In broth at 20° C., abundant flocculent growths are evident in twenty to twenty-four hours, but no general turbidity. In forty-eight hours a thick, dull-white, mould-like membrane is formed on the surface, and flocculent cotton-wool-like masses have developed below, and occasional flocks of the same kind float in the otherwise clear broth. The floating membrane thickens rapidly, but the submerged flocks do not develop so quickly, evidently owing to the want of oxygen below the surface. As the membrane breaks up, flocks fall through the liquid and increase the cottony deposit, but otherwise no further change occurs up to the sixth or seventh day, when all growth ceases. The liquid remains clear throughout, evidently because the filaments do not break up into motile bacilli. All attempts to find cilia—by Loeffler's method as well as by the improved form of it given by Alfred Fischer\*—have failed, and there can be little doubt that, like anthrax and some other forms, no such organs are present, common as they may be in the group of schizomycetes generally.†

Cultivated in milk at 20° C., no changes are visible at first, but it grows slowly and gradually dissolves the casein, with an alkaline reaction. The liquor becomes yellow, and copious networks of filaments are to be found in the cream above.

\* See Pringsheim's Jahrb., 1894, p. 187.

† See Fischer, *loc. cit.*

In a 3 per cent. solution of glucose, to which a little broth and Liebig's extract (mere traces) are added, the cultures at 20° C. show rapid growth in twenty-four hours as white cotton-wool-like flocks, resembling those formed in broth.

Further research has shown that it grows readily in and on all ordinary media, at all temperatures from 12—15° C. up to 27—30° C., the optimum being near 25° C. But it is also capable of slow growth at temperatures below 7° or 8° C., and even above 38—39° C. for a short time. It is not strictly aërobic, but nearly so, and yields no pigment or other pronounced excretion.

I owe to the kindness of Dr. W. S. Lazarus-Barlow, of the Pathological Laboratory, Cambridge, the following proof that this organism is not pathogenic:—

"The question whether the bacillus were pathogenic or non-pathogenic was investigated on mice and on guinea-pigs. Beef-broth-cultures of ages varying from one to seventy-eight days, and in quantities of 2 c.c. in the case of the mice and 20 c.c. in the case of guinea-pigs, were used. The mice were inoculated sub-cutaneously, and the guinea-pigs intra-peritoneally. In no animal was any deviation from the normal observed except such as were directly and solely attributable to the introduction of the aseptic needle of the syringe. The bacillus is therefore non-pathogenic. This conclusion is further supported by the fact that the bacillus grows better at the room-temperature than at the body-temperature."

I have to record my thanks not only to Dr. Lazarus-Barlow for the trouble he took in the above investigation, but also to Professor Roy for permitting the investigation in his laboratory.

Characters of *Bacillus ramosus* (Fraenkel).

|                                       |                                                                                                                                                                                                                                                                                                                                                                                     |
|---------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Habitat .....                         | Thames water, especially in autumn and winter, but also in summer.                                                                                                                                                                                                                                                                                                                  |
| Morphology .....                      | As long, straight, or curved filaments, often twisted into tresses; subsequently breaking up into segments (bacilli). Diameter of filaments = 1.75 $\mu$ , length 20, 50 to 100 $\mu$ or more. Bacilli 3 to 6 $\mu$ $\times$ 1.75 $\mu$ , with rounded ends, often in long chains. Forms spores = 1.75 to 2 $\mu$ $\times$ 1.5 $\mu$ .                                              |
| Movements .....                       | Not motile, but the growing filaments exhibit slight nutation movements.                                                                                                                                                                                                                                                                                                            |
| Colonies on gelatine plates at 15° C. | Appear in two days as circular, ill-defined, mould-like, white, rapidly-growing membranes. In three days may attain 25 mm. diameter; in ninety-six hours = 50 mm.; in six days 75 mm. or more. Mycelial-like membrane radiately veined with thicker and thinner strands of woven filaments, like a root-system or myxomycete plasmodium: slowly liquefies from about the third day. |

Characters of *Bacillus ramosus* (Fraenkel)—continued.

|                                           |                                                                                                                                                                                                                                                                                                                                    |
|-------------------------------------------|------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Streak - cultures on gelatine at 15° C.   | Spreads rapidly as a white, liquefying membrane, consisting of irregularly radiating off-shoots from streak.                                                                                                                                                                                                                       |
| Stab-cultures in gelatine at 15° C.       | Fine white colonies along the line of puncture, giving off white, silky, radiating, and branched filaments, the whole looking like a root with root-hairs, and later like an inverted fir-tree. Liquefaction slow from above downwards.                                                                                            |
| Streak - cultures on agar at 20° C.       | Growth much the same as on gelatine, but more opaque and chalky white, granulated. In three days develops spores, and the filaments break up into a pasty mass.                                                                                                                                                                    |
| On potato at 20° C...                     | Spreads as a dull, pasty, white thick layer. In about three days becomes wrinkled and powdery, owing to breaking up of filaments and development of spores.                                                                                                                                                                        |
| In broth at 20° C....                     | Rapidly develops a white, superficial, dull, mould-like membrane, and flocks of deposit like cotton wool below. Does not give rise to any general turbidity.                                                                                                                                                                       |
| In 3 per cent. glucose solution at 20° C. | Rapid growth in twenty-four hours as white flocks, like those in broth, but feebler. Tends to form a slight ring, but no veil. No trace of fermentation. In ten days there are spores, but not numerous, and many segments have none.                                                                                              |
| In milk at 20° C.....                     | No visible signs in forty-eight hours, but thence onwards the casein is slowly dissolved from above downwards without precipitation, leaving a brown-yellow liquor below the cream, in which copious networks of filaments are developed. The reaction is strongly alkaline.                                                       |
| Requirements as to air                    | Aërobic, but partially anaërobic also.                                                                                                                                                                                                                                                                                             |
| Temperature.....                          | Grows best about 25° C., but will grow at any temperature between 6—12° C. and 38—39° C. The spores may be heated to 100° C. for a minute, but are killed by 2—5 minutes boiling. They withstand drying at 80° C., and germinate normally after it; they may also be kept at 60° C. for twenty-four hours without apparent injury. |
| Pigment .....                             | None.                                                                                                                                                                                                                                                                                                                              |
| Pathogenicity .....                       | Non-pathogenic for mice or guinea-pigs.                                                                                                                                                                                                                                                                                            |

## Classification.

It is obvious from the foregoing researches that this schizomycete comes into the group of true bacillar forms. If we adopt the very exhaustive classification attempted by Saccardo,\* it goes into the

\* 'Sylloge Fungorum,' 1889, vol. 8, p. 924. For an examination of this and other systems of classification see 'Annals of Botany,' vol. 6, 1892, p. 103.

second of his three sub-families—the *Baculogenæ*—though it is somewhat difficult to accept his statement that the bacillar segment, and not the filament, is the primary form. Be this as it may, however, this Thames schizomycete must be excluded from the *Trichogenæ*, Saccardo's first sub-family, embracing the genera *Crenothrix*, *Cladothrix*, *Beggiatoa*, and their allies, as well as from his third group—the *Coccogenæ*—which includes *Micrococcus*, *Sarcina*, *Streptococcus*, &c., &c.

Among the *Baculogenæ*—which includes the genera *Bacillus*, *Clostridium*, *Vibrio*, *Bacterium*, and their allies—it clearly comes under the *Endosporeæ*, a group embracing those genera which produce definite spores in the rods—*Pasteuria*, *Thiodictyon*, *Mantegazzia*, *Bacillus*, *Pasteurella*, *Clostridium*, *Cornilia*, *Vibrio*, *Spirillum*, and *Spiromonas*.

*Pasteuria* is excluded by its mode of longitudinal division. *Thiodictyon* by its cœnobial colonies. *Spirillum* and *Spiromonas* by their regular corkscrew-like twisting. *Cornilia* and *Vibrio* by their peculiar spore-formation and other characters; whence we are reduced to the true bacillæ (*Eubacillæ*).

This sub-group includes the genera *Mantegazzia*, with fusiform rodlets; *Bacillus*, with cylindrical rodlets; and *Pasteurella* and *Clostridium*, each of which has peculiarities of spore-formation different from those described.

This brings us to the genus *Bacillus* proper.

The further subdivision of this genus is still very unsatisfactory. Saccardo adopts a series of sections based on the habitats, whether anthrophobic, zoobic, pyogenic, zymogenic, saprophytic, and endophytic, subdividing further, according to the organs the pathogenic forms occur in, the behaviour towards gelatine, whether they form pigments, and so on.

Accepting this provisionally, the species in question comes under the saprophytic section. Here we find forms peculiar to the surface of the human body, or to the blood of cadavers, &c., and others especially characteristic of putrefaction, or stagnant water, and so on.

Clearly the present species is a *Bacillus* found in water, not necessarily stagnant, however, and saprophytic. It is, moreover, ærobic, achroic, and liquefies gelatine at ordinary temperatures, as we have seen.

If we now inquire what species this form belongs to, there are several decided and well-marked characters to guide us. There are not many schizomycetes known which are so persistently filamentous, and form such large, mycelium-like colonies on gelatine, and whose cells measure over  $1.5\ \mu$  in thickness; which, moreover, easily form large, oval spores, and, finally, have such a characteristic stab-culture as this one.

Taking the large size of the bacillar segments as our primary clue, the vast majority of known bacillar or filamentous schizomycetes rarely exceed  $0.5\ \mu$  in diameter, and very few surpass  $1\ \mu$ .

Of these, we may at once dismiss Van Tieghem's giant *B. crassus*,  $4\ \mu$  in diameter, and the thickest form known,\* and even De Bary's *B. megaterium*,  $2.5\ \mu$  in diameter, is much thicker than the form I am discussing.

A small number of species, such as *B. Brassica*, *B. tumescens*, *B. Zopfi*, *B. Mallei*, *B. ascoformans*, *B. indigogenus*, approach our form in dimensions, but their other characters at once separate them.

*B. anthracis* presents several suggestive resemblances to the present schizomycete, but it is thinner and smaller altogether, and its spores, in addition to being smaller, germinate differently, and have different temperature requirements. Moreover, though the stab-cultures of *B. anthracis* resemble those of the Thames form, the gelatine plate cultures are different. The spores of the former are also much more sensitive to light, and the Thames species is not pathogenic. Maschek's "*Bäumchen-bacillus*," though it forms a dendroid stab-culture, differs in almost all its other characters, and ferments saccharine solutions.

As regards the tendency to form mycelium-like colonies on the plates, several species found in water and elsewhere resemble the Thames species, e.g., *B. radiatus*, *B. muscoides*, *B. polypiformis*, *B. mycoides*, *B. ramosus*, *B. implexus*.†

Of these, we may rapidly dismiss *B. muscoides*, *B. polypiformis*, and *B. radiatus*, since they are strictly anaërobic, and will not grow under ordinary conditions, to say nothing of their many other specific differences.

*B. implexus* is too thin, and Zimmermann's curt description of the colonies suffices to show that it is totally unlike the Thames form. *B. mycoides* is also too thin, but it forms oval spores, and the plate-cultures resemble those of the form in question in many particulars. The potato-cultures differ, however. More information is wanted about this species, which is said to be very common in soil and water.

There remains *B. ramosus*. I understand by this specific name the form described by Eisenberg‡ under that name, and known in Germany as the "*Wurzel-bacillus*." Crookshank§ gives the latter under the name *B. figurans*, and as a synonym for Flügge's *B. mycoides*, but Eisenberg gives quite a different description for the latter, and quotes Flügge and Zimmermann in support of his statements.

\* Quoted by De Bary, 'Lectures on Bacteria,' p. 3. Of course I leave out of account the genera *Crenothrix*, *Beggiatoa*, &c.

† See Lustig, 'Diagnostik der Bakterien des Wassers,' 1893, p. 82.

‡ *Loc. cit.*, p. 126.

§ 'Manual of Bacteriology,' 1887, p. 311.

Saccardo\* gives under *B. Praussnitzii*, a synonym *B. ramosus liquefaciens* of Flügge; whether this is the same I cannot determine from the very short and vague description.

Macé† gives *B. radicosus* as the "wurzel-bacillus," but his description is too vague to enable me to determine whether it is the *B. ramosus* of Eisenberg; he does not intend it for Flügge's *B. mycoides*, however, since he describes that separately. Zimmermann also gives it as *B. radicosus* and Tataroff as *B. radiciformis*.‡

The Franklands,§ who found the same, or a very similar, form common in the Thames and Lea, regard their species as the *B. ramosus* of Eisenberg and Fraenkel, and as the "wurzel-bacillus," and Lustig|| gives the descriptions separately, but remarks on their probable identity.

I have little doubt that the species I have isolated from the Thames is Eisenberg's and Lustig's *B. ramosus*; that it is the "wurzel-bacillus" of Fraenkel and Eisenberg; and that it is identical with that found by the Franklands, though the figures given by the latter are not clear enough to identify it by.

In the size of the rodlets and filaments, mycelium-like plate-colonies, root-like stab-cultures, behaviour on agar and potatoes, characters of the spores, and, indeed, in almost every detail of which I can get information, my Thames form agrees with Fraenkel's *B. ramosus*, the "wurzel-bacillus" of the Germans. What the forms described by Macé, Crookshank, Saccardo, and Flügge may be is not clear, and probably some confusion exists here.

It seems pertinent to remark here that many bacteriologists are not sufficiently careful in all cases to look up the synonyms of the forms they describe, though this precaution is really more necessary in the deplorable state of their literature than probably in any other department of biology.

#### Germination of Spores.

As the mass cultures prove, the spores germinate readily at all ordinary temperatures, whence was to be inferred that no extraordinary difficulty ought to be incurred in observing the process, provided I could succeed in isolating a single spore under a sufficiently high power in a hanging drop, and such turned out to be the case.

The methods and apparatus employed were similar to those I had

\* *Loc. cit.*, p. 989 (No. 202).

† *Loc. cit.*, p. 610.

‡ Zimmermann, 'Die Bakt. unserer Trink-und Nutz-wässer,' p. 30. Tataroff, Dorpat, 1891.

§ *Loc. cit.*, p. 388.

|| *Loc. cit.*, pp. 96, 97.

used previously in isolating the yeasts and bacteria of the "ginger-beer plant."\*

A small drop of nutrient gelatine or of broth, properly infected with ripe spores from an agar tube, was placed on the under surface of a sterilised cover-slip, and the latter luted by means of sterile gelatine, so as to serve as the lid to one of the glass cultivating chambers, properly sterilised, the floor of which consisted of a glass slip, luted by means of melted paraffin. Over the floor of the chamber was spread a layer of sterilised distilled water, and the tubular arms were plugged with cotton-wool saturated with water, all properly sterilised. The hanging drop, selected by examining a number and choosing that which appeared to contain only one, or at most a very few spores, separated by a sufficient distance, had to be sufficiently flat to enable the lens to penetrate through its depth.

#### *Methods.*

As regards my own practice in making these cell-cultures, the following note may be of use to other students; though of course different workers may operate differently.

I first sterilise the plugged cells in the hot-air steriliser. When these are nearly cool enough to handle, I heat the quartz or glass floor of the cell between two plates of talc (about  $3 \times 6$  inches) held in a large bunsen flame, and allow it to cool slightly. The cell is then placed in position on its floor, and a small block of high-melting paraffin—cooled after sterilising—is placed just outside the cell. The temperature is still high enough to melt this, and the liquid runs in by capillarity, and solidifies as the whole cools, cementing the cell to its floor.

When the culture is to be made I take such a prepared cell—several can be prepared and kept in sterile glass-covered dishes—and have ready the following: cover slips, a tube of infected gelatine (or other medium), platinum loops, sterile stiff gelatine, forceps, sterile water, and any convenient rest, such as a small ring of brass.

First, the necessary water is placed in the plugged cell, care being taken to wet the cotton plugs and that some water shall be retained on the inner side of each, whether a layer is spread over the floor or not.

Second, a cover-slip is sterilised between the talc-plates, and while it is cooling the platinum loops are held in the flame and set aside to cool.

Third, the cool sterile cover-slip is removed with the forceps, and laid on the brass ring support. Then the drop is quickly put on the centre of the slip, and the latter inverted on to the cell, so that the drop hangs over the centre.

\* See 'Phil. Trans.,' vol. 183, 1892, B, p. 125.

Fourth, the slip with its hanging drop is now cemented as a roof to the cell by running melted sterile gelatine between it and the latter.

A little practice enables one to prepare such a culture in a few minutes, and very rarely need one go wrong if care is taken. The chief difficulties are with fluid drops, or very dilute gelatine, since they are apt to spread and run over the glass, especially when the air is moist and condenses quickly on the glass surface; in these cases, however, a little experience enables one to avoid letting the cover-slip get too cold before the drop is attached—though, of course the opposite danger has to be guarded against.

Having isolated a single spore, suspended in the drop of nutrient medium beneath the objective—the observations were made with Swift's 1/20th and Zeiss' 1/12th oil immersion, and with Zeiss' E, occ. 4—a drawing of the freshly sown spore was at once made. The culture was then left, with a bell-jar, darkened with black-paper over the whole, at a temperature of 15° to 20° C,\* and further observations and drawings made at intervals. Naturally there were many failures, especially with the high-power immersion lens, and the following successful series were only obtained at intervals from cultures in which the thickness of the cover-slip, and of the hanging drop, the sufficient isolation of the spore, and the normal germination and further progress were suitable, and where no sudden changes of temperature interfered to check the growth, dry up the gelatine, or cause inconvenient condensations of moisture in the chamber, the relatively large size of which has again proved advantageous owing to the abundant supply of oxygen it ensures.

Under the conditions referred to, the spore without materially changing its ovoid shape begins to swell somewhat rapidly, and in from one to two hours has increased its dimensions from about  $1.5 \times 2 \mu$  to  $2 \times 2.5 \mu$  or more. As it does this the brilliant oil-drop-like contents become duller and more hyaline—like ground glass—and the sharply marked, almost black membrane, gradually loses some of the firmness of its contour, until it appears as a thinner limiting membrane. At the same time it becomes surrounded by an almost imperceptible pale halo-like investment which appears to be derived from the deliquescence of its most external layers, probably into a soft, transparent, swollen jelly. (Cf. figs. 9—12.)

In the course of the next one to two hours or so, the spore appears to be elongating. Close observation shows that this is due to a thinning out of the membrane at one of the ends, and soon afterwards the thinned out wall gives way, and the pale, hyaline, apparently homogeneous protoplasm, enveloped in an exceedingly

\* These temperatures are somewhat low, and later results were got at 25—26° C. and higher.

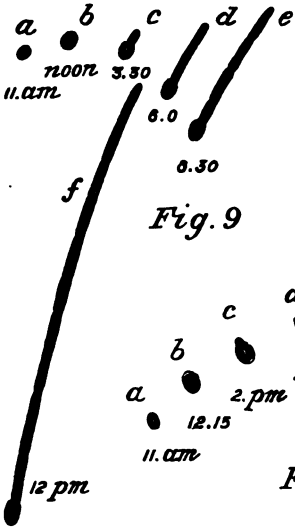


Fig. 9

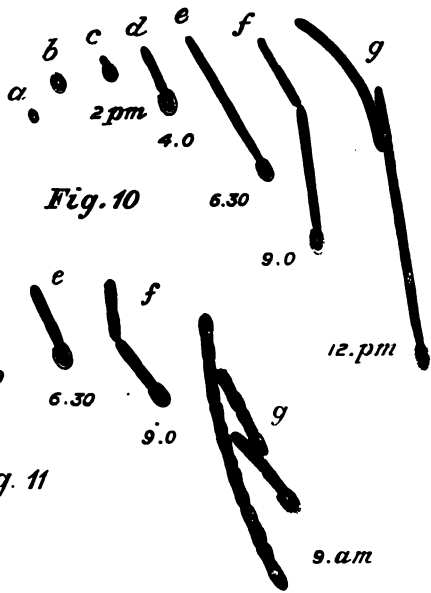


Fig. 10

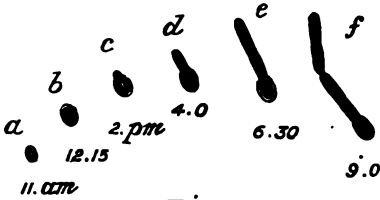


Fig. 11

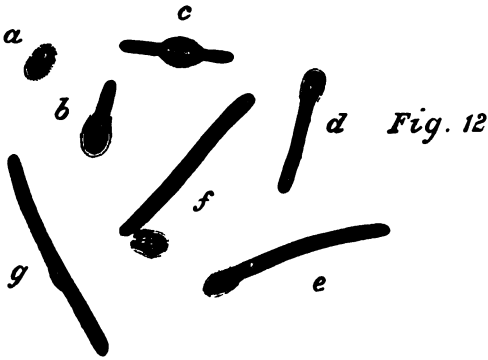


Fig. 12

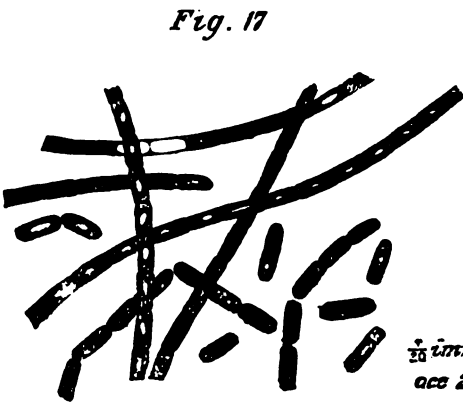


Fig. 17

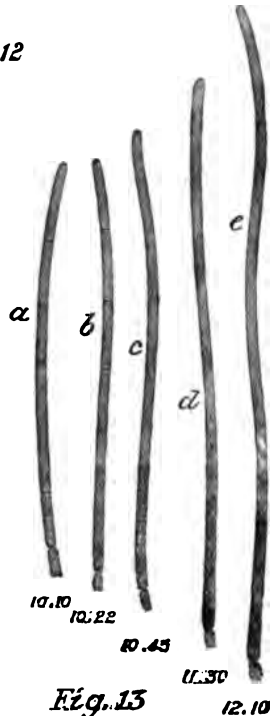


Fig. 13

tenuous membrane, pushes its way out and grows as a blunt rod, about  $1.75\ \mu$  broad, with rounded apex, in the direction of the longer axis of the spore. (Figs. 9 c, 10 c, 11 c.)

In about four or five hours from the beginning of germination this straight rodlet has attained a length equal to twice that of the spore, and two or three hours later it has a length of approximately four times that of the spore (figs. 9 e, 10 d, 11 e), the membrane of which is still observable usually as a cap at the proximal rounded end of the rod. (Same figures and fig. 12 b, d, e.)

The above is by far the commonest mode of germination, but in some cases this normal condition of affairs is so far modified that both ends of the spore are softened, and each gives rise to a germinal rodlet (fig. 12 c and g) in which case the remains of the spore-membrane may be found either encircling the germinal rodlet, much as a napkin ring does a rolled up serviette (fig. 12 c), or ruptured at one side and merely adhering to the rodlet as in fig. 12 g. Occasionally, rodlets which have germinated out in the normal mode are found with the collapsed membrane lying loosely at one end, evidently having been thrown off, as in fig. 12 f; this seems to occur rather frequently in the later stages of germination in broth-cultures. All these phenomena point to the elastic nature of the thin, but tough, spore membrane.

When the germinal rod or filament has attained a length equal to about four or five times that of the swollen spore, the first division wall is usually seen in the centre (figs. 11 e, 12 d, and e). Whether the case illustrated in fig. 10 d is really an exception to this rule, or whether the apparent septum closer to the spore was really the rim of the burst spore (*cf.* fig. 12 e), I cannot be certain; from the fact that I could not trace it in the next stage (fig. 10 e) it seems likely that the latter supposition is the correct one, and in any case the rule is that the first transverse septum divides the whole germinal filament into two cells approximately, but not necessarily exactly, equal in length, and measuring about 3 to  $5\ \mu$  long by  $1.75\ \mu$  broad.

The germination now rapidly proceeds by the growth in length of the stiff and nearly straight filament along its whole course, and in about six to eight hours from the commencement of the swelling of the spore, the filament is from 8 to 10 times as long as the spore (figs. 9 e, 10 e, 11 f), and each of the two cells into which it was segmented by the first transverse septum has been again bisected by a septum, thus cutting the filament into four segments (figs. as before).

The growth is therefore not merely terminal, but intercalary along the whole filament. I have spoken of the latter as being stiff and nearly straight; the qualifying word is necessary because close observation of broth cultures shows that the rounded apex performs

very slow and very slight movements, due to almost imperceptible curvatures of the filament. These can be traced by watching the growth for a few minutes under the eye-piece micrometer, and are interesting as indications of slight movements of nutation. As I shall have to speak of these, and the measurements I have made of them and of the rate of growth, when describing the behaviour of the older filaments, however, they may be passed over here simply with the remark that they do occur.

Even at this early stage, or a little later, the filaments, especially in broth cultures, may begin to break across, generally, but not always, at the oldest septum (figs. 11 *e*, and 10 *f*), though this process is by no means the common or normal one in rapidly growing cultures (fig. 9 *f*).

Sooner or later, however, some amount of fission does occur, and the further behaviour of the two or more broken filaments is interesting, as throwing light on the formation of the tresses and strands, and their false branches, which we meet with later.

Fig. 11 *f* and *g* is an instructive case in point. As the first stage shows, the filament broke at the oldest septum when little more than eight times as long as the spore, and consisting of four segments, and when only ten hours old. The lowermost two-celled segment, proximal to the spore, then slipped its now rounded tip to the right, and continued growing; the uppermost, free and distal, two-celled segment also grew so that both ends travelled further apart, and in such a way that the tip of the proximal segment slipped along its right side. Further divisions followed on growth in both cases, and the right hand (proximal) segment again broke during the night at its oldest segment, when it had divided into four cells, the ends again slipping over one another, so that at nine o'clock next morning (twelve hours later) the state of affairs was as in fig. 11 *g*. This was a gelatine culture, not growing very rapidly, but I shall have to revert to these phenomena later in discussing the process of growth in broth cultures.

It is already intelligible, however, that this slipping of the broken filaments one over the other, each portion growing independently soon gives rise to strands or tresses compounded of numerous filaments, the free ends of which stand off as false branches (the same thing is beginning in fig. 10 *f* and *g*), and so the initiation of a colony, with offshoots radiating in all directions into the surrounding medium, is established.

From the fact that well isolated spores, freely suspended in a drop of broth, give rise to filaments which may grow to many hundred times the length of the spore before any breaking across occurs, and that such breakage is very apt to occur when one such long filament eventually abuts on another, or on some obstacle which bends the

filament, I am strongly inclined to refer the breakage in the above cases to mechanical causes, *e.g.*, the resistance offered by the gelatine. Indeed, I have watched a long filament in broth, thus abutting on another, and seen it curve until bent into more than half a circle (evidently owing to intercalary growth between the two relatively fixed ends) and then suddenly snap at one of the septa, and the two freed ends dart past each other owing to the elasticity of the filament which had given way. At the same time, there is a period when the (much older) filaments are peculiarly apt to break up into shorter segments, preliminary to spore formation, and this must be due to other causes than mere fracture due to mechanical pressures or tensions, indeed, even in the above cases I do not suppose that the mechanical strains do more than determine the sudden rupture at a septum already prepared to give way, but which probably would not yet have done so had the filament been able to continue its onward growth in a more or less straight line.

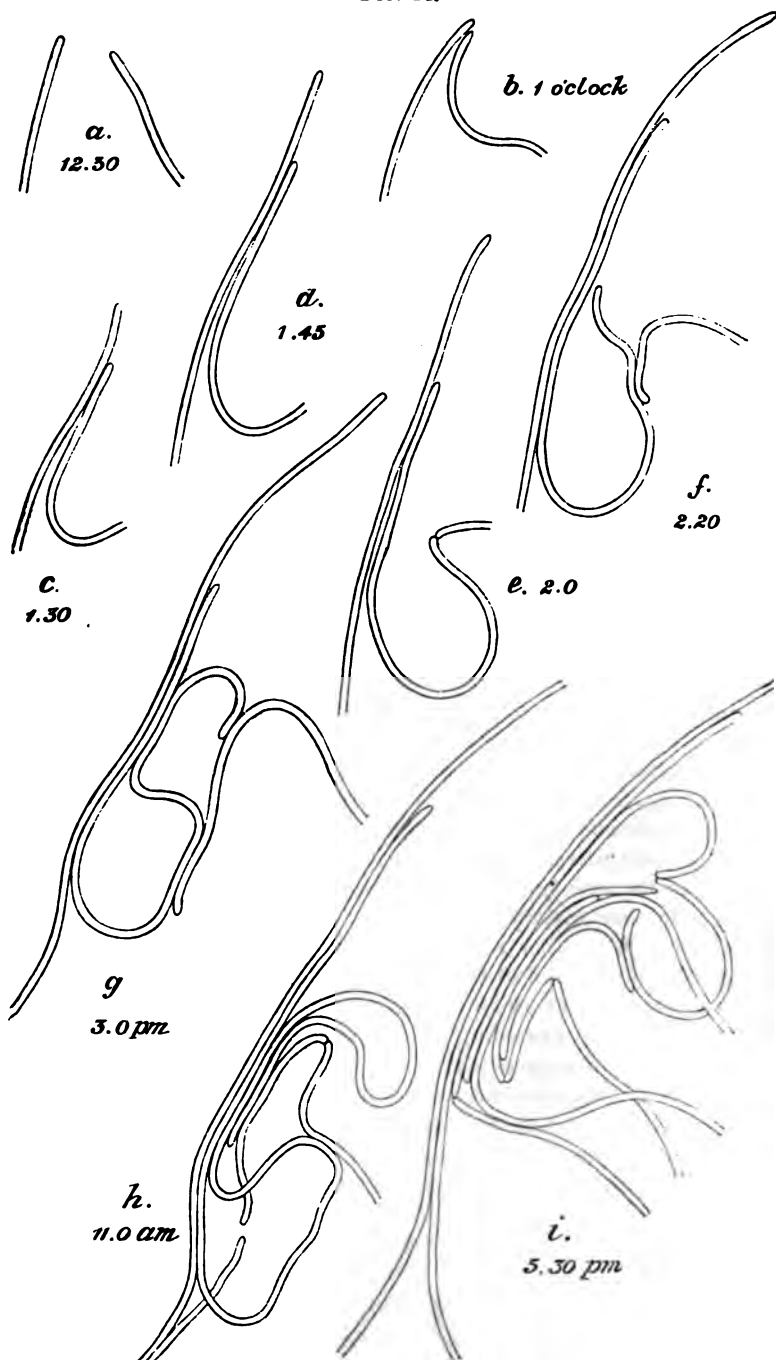
#### *Formation of Tresses.*

I have already described one mode of formation of the strands or tresses composed of numerous filaments lying parallel to one another, by means of the sliding growth of the two pieces of a ruptured filament one over the other.

Perhaps an even more common process is the modification of this figured in fig. 14 *a* to *i*, a series followed under the microscope (E occ. 4) on a broth culture at 17.5° to 19.5° C. The culture was the same as that from which figs. 9 and 13 were obtained, and, in fact just as fig. 13 represents particulars of the growth of one of the filaments developed from the spore of fig. 9 (and traced in that figure for the first thirteen hours of germination) followed from the twenty-fourth to the twenty-sixth hour from germination, so this fig. 14 takes up the further history of the same filament from twenty minutes later through another four hours; for the left-hand filament in fig. 14 *a* is the upper end of the filament *e*, fig. 13.

We left this at 12.10 P.M., having traced its growth and nutations during the preceding two hours. At 12.30 the distal end of another filament, segmented off lower down, was seen to be curving over towards the one referred to, and half-an-hour later (fig. 14 *b*) the tips of both were in contact and growing up alongside one another. I was strongly inclined to suspect the existence of a slight attraction between them from the relatively strong curvatures towards each other which they exhibited previous to contact, but could not be sure, and thought possibly the phenomenon was of the nature of a merely physical action. During the next hour (fig. 14 *c*, *d*, *e*) a longer and longer stretch of the growing, curved, right-hand filament laid itself

FIG. 14.



close to the rapidly elongating left-hand one, and then an interesting and instructive phenomenon ensued.

The long, curved, right-hand filament suddenly snapped at one of its septa, and shortly after the drawing (e) was made the two ends began to glide one over the other, and at 2.20 had assumed the positions shown in fig. 14 f. The rapid growth of the extreme right-hand filament caused it to curve strongly, and this marked curvature increased, until at 3 P.M. the large curve looked as if the filament was pressing the S-shaped one, trapped between it and the left-hand spring-like filament, elastically against the latter. (Fig. 14 g).

At any rate, the free end of this spring-like right-hand filament slipped soon after over the lower half of the S to the left, while its upper part went on, so to speak, pressing the upper limb of the S also to the left, until it had trapped it close up against the back of the S (fig. 14 h), and then it also snapped.

Comparison of the figures will convince us that the series of contiguous parallel filaments making up the middle portion of fig. 14 i are brought together by these repeated doublings up of the snapped filaments, pressing up close to one another. The series also shows pretty clearly how pronounced is the intercalary growth, *e.g.*, along the curved upper and lower line of the S-shaped piece in figs. g, h, and i.

The process may be summarised as consisting in (1) the looping of filaments, which go on elongating by intercalary growth, the two limbs of the loop being doubled one on to the other, possibly by pressure of other filaments; (2) the snapping of the same when the doubling up becomes very sharp; and (3) the straightening out of the broken pieces side by side, and further parallel growth in close contiguity as a strand or tress.

The case followed is a relatively simple one. In larger and older colonies, and in cultures where many colonies are growing together, the tresses may consist of scores and even hundreds of such parallel filaments. Moreover, they are not necessarily arranged in the flat, ribbon-like manner depicted in these young and small cultures growing in a thin layer of broth, but may be thick and of various sections.

It will also be understood how—quite apart from any question of mutual attraction between filament and filament—the fact of the two terminal stretches of a long and otherwise free loop being relatively fixed as they grow along the sides of such a strand, may force the loop to make coils of various kinds during the elongation due to intercalary growth, and it is thus I explain the extremely common occurrence of these coils in large cultures.

#### *Development of Spores.*

Perhaps the most interesting—as it certainly has been one of the most difficult—series of observations I have made on this schizo-

mycete, has been the tracing of the successive stages of development of the spores under high-power objectives; and here again I have had the satisfaction of tracing the whole process in one of the filaments of the same series as those figured in figs. 9, 13, and 14.

The strand drawn in fig. 14 *i*, was left at 5.30 P.M. at a stage too complex to follow further, and when its age—reckoned from the swelling of the spore which produced the filaments—was  $30\frac{1}{2}$  hours. At 9 o'clock next morning these filaments showed evidence of proceeding to the development of spores, and before night every filament had a completely developed spore in each of its numerous segments, or if any of the segments remained barren they must have been very few indeed.

I had already traced the development of the spores, but it was obviously an interesting task to do this in one of these filaments which I had kept so continuously under observation, and the following description refers to a portion developed during the night from the further growth of one of the loose filaments in fig. 14 *i*.

To do this I exchanged the objective (E, occ. 4) for a  $1/12$ th oil immersion, and at 10.30 A.M. made the notes of the first changes which initiate the formation of the endogenous spores. This culture which, as we have seen, was in broth at  $20^{\circ}$  C.\* (occasionally lower), was now aged  $47\frac{1}{2}$  hours from the moment of drawing the spores (fig. 9 *a*).

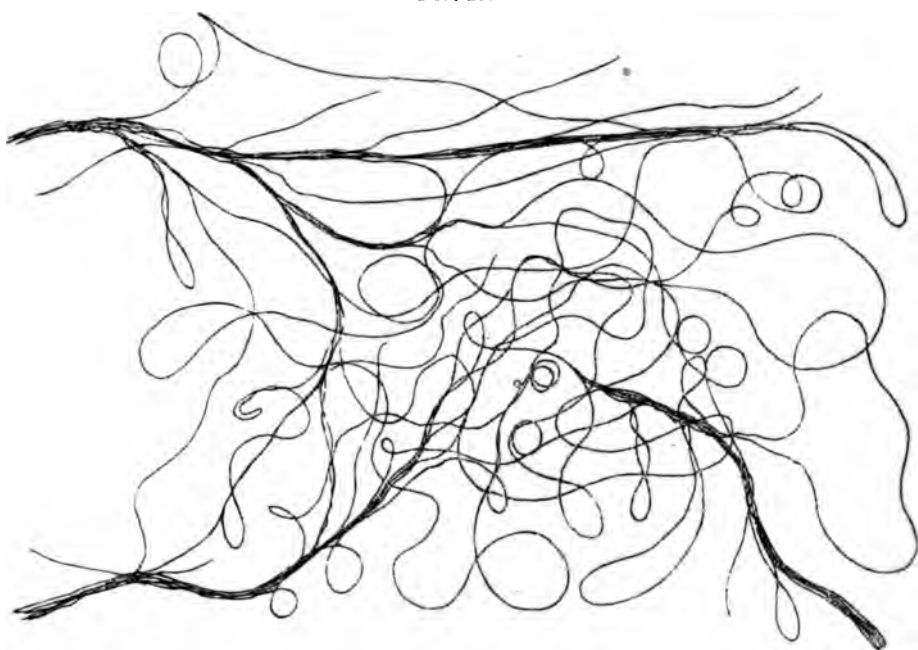
The first indication of spore-formation consists in the appearance of brilliant points in the cells which have now ceased to grow or divide. There may be one or two or more of these bright spots in each cell, and the septa show signs which seem to precede a tendency to split. They look swollen and bright, but I am not sure whether this is really due to such a change in them, or whether it is owing to a slight contraction of the protoplasm from contact with the walls.

During the course of the next three or four hours the bright, oily-looking globules† gradually enlarge, and, where more than one occur in a cell, some may run together into one bright mass, which slowly balls itself together in the centre of the cell, and, in the case described, there was one such mass definitely established as an oval body in each segment at 3.30 P.M. In the course of the next hour this bright, oval body had increased in size, apparently at the expence of the cell-protoplasm in which it lay, and constituted a definite spore. This slowly acquired a more and more pronounced spore-membrane towards

\* This, like all the temperatures in this part of the work, refers to the air temperature: a point of some importance later on.

† They are not oil, because they stain with methylene blue and other dyes; it is more likely that they are the chromatin granules referred to in Hueppe, 'Methoden der Bakt.-forsch.', 5th edit., 1891, p. 154.

FIG. 15.



evening, and by 10 P.M., or thereabouts, was completed in all respects as a ripe spore like that started with sixty-nine hours previously.

A still better series of stages observed in the development of the spore is shown in fig. 18. The filaments of a very vigorous culture in broth at 20° C. were passing over to the development of spores in nineteen to twenty hours after sowing, and at 10.55 A.M. the cells, about twice as long as broad, had their protoplasm studded with the minute brilliant points already referred to (fig. 18 *a*). These bright masses stain deeply with methylene blue, as said, and are probably of the nature of the chromatin granules referred to in works on bacteriology.\* They also slowly change their positions in the protoplasm, and at 11.40 A.M. had changed in numbers, positions, and sizes, as shown in fig. 18 *b*. These changes slowly proceeded, in the case under observation, until all the bright substance was aggregated near the centre of the protoplasm into a more or less definite oval body (fig. 18 *c*) which is evidently the incipient spore. This was at 9 P.M. During the night these masses had each become larger, more defined, and had clothed itself with a distinct cell wall (fig. 18 *d*). Each cell now contained one distinct oval spore, and was more cut off from its neighbour by a tendency to rounding

\* See Hueppe, 'Methoden,' *loc. cit.*

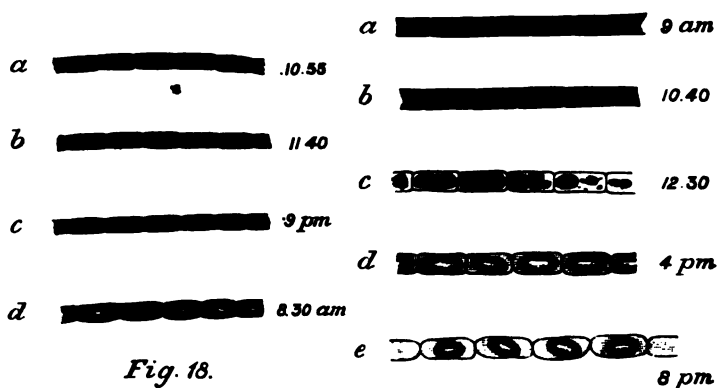


Fig. 18.

Fig. 19.

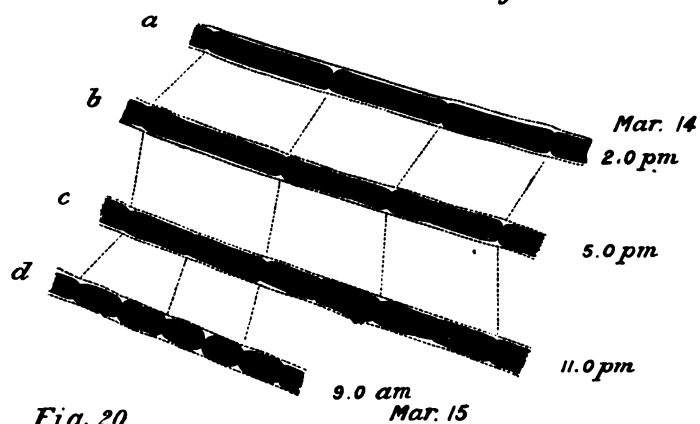


Fig. 20.

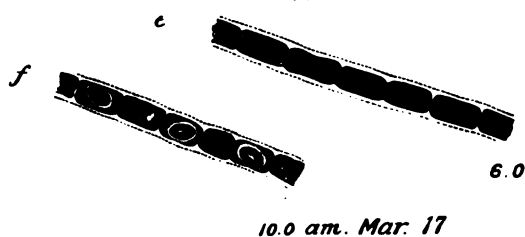
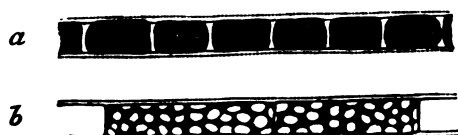


Fig. 21.



Fig. 22.



off. The ripe spore does not contain all the cell-contents, but lies embedded in a matrix of something remaining over from the cell-protoplasm. This might also be inferred from the double staining of which such preparations are capable.

In this case, therefore, the whole life-history—from spore to spore—was gone through in a little over forty hours.

Another equally interesting and rapid case of spore-formation is figured in fig. 19, where the changes in the granules and the gradual isolation of the spore mother-cells are again evident traced under a still higher power (1/20th oil immersion).

In fig. 20 I have shown a series in a case which presents some points of extreme interest, which, it seems to me, would be well worth special study.

A culture which had been exposed to blue light, and was much retarded, began to show signs of spore-formation at 2 p.m.—the filaments being then sixty-four hours from sowing. It was now in ordinary daylight, but I have no records of the details of temperature.

The peculiarities observable are (1) the very late appearance of the bright granules; (2) that they were first developed in cells much longer than usual; (3) their slow changes of position during several hours (fig. 20 *a-c*); (4) the subsequent division of the cells to the short form characteristic of spore-mother-cells (fig. 20 *c, d*); and (5) the incomplete character of the spores when they did at last appear more definitely in some of the cells.

I cannot avoid the conclusion that the blue light had something to do with this retardation and hindrance of spore-formation—an observation quite in accordance with some earlier ones on the development of spores in fungi\*—and, as said, believe the subject well worth further study.

That the bodies above described are true spores is proved by their behaviour towards staining and other reagents, their resistance to high temperatures,† and above all by their germination which, as we have seen, I have observed and traced stage by stage.

They refuse to stain by all the ordinary methods for staining the bacilli, but stand out from the coloured protoplasm in such cases like brilliant colourless oil drops; on the other hand they stain fairly easily by the usual methods for spores, and especially with aniline-fuchsin or anilin-methyl violet, after being passed 10 times through the flame.

The ripe spores from potato- or agar-cultures germinated readily

\* See 'Bot. Zeitung,' 1885, p. 6, where L. Klein confirms Rindfleisch in the observation that the development of the conidia of *Botrytis* is inhibited by blue light and by ordinary daylight, but not by red light.

† See pp. 350 and 351 for more detailed proofs of this.

in broth in which they were heated to  $80^{\circ}$  C. for two hours, and somewhat more slowly in the same medium in which they were boiled for a minute; but after five minutes boiling they appeared to be all killed, since none germinated out in six days at  $20^{\circ}$  C.

*Measurements of Growth of the Rods and Filaments.*

As will be evident from what has been said above concerning the growth of the germinal filaments, they elongate in nearly a straight line so long as they are free to do so and meet with no mechanical obstruction. This fact, and the obviously rapid growth, led me to a method for measuring the rate of elongation; and not only did I succeed in doing this efficiently, but these growth measurements carried me on much further and to some unexpected and interesting results in another connection.

On placing the eye-piece micrometer so that its vertical division crossed the long axis of a filament shorter than the scale, it was easy to observe the gradual extension of the filament as its ends passed over the divisions. The value of each division was determined beforehand, by examining a stage-micrometer with the same combination as I used for the measurements.

Having selected a nearly straight filament which extended over twenty-seven of the fifty divisions on the scale, and having determined that each division was equal to  $3\mu$  for the power—Zeiss E occ. 2—employed, it was evident the filament was  $81\mu$  long.

It was growing in broth at  $16^{\circ}$  C. when put under observation at 10.10 A.M., and was watched for two hours, during which period the thermometer rose from  $18^{\circ}$  C. to  $20^{\circ}$  C.\* The microscope stood under a shaded bell-jar at a south window, and the day was cloudy and dull.

At 10.22 A.M.—i.e., twelve minutes after measuring the filament—it had elongated so as to cover thirty divisions instead of twenty-seven. In other words it had grown  $9\mu$  longer (fig. 13 a, b) and had slightly altered its slight curvature. At 10.45 it had grown another  $12\mu$  (fig. 13 c); at 11.30 it was longer by  $18\mu$ , and at 12.10 its elongation amounted to  $24\mu$  further.

That is to say, in the interval from 10.10 A.M. to 12.10 P.M. (two hours) the total growth in length of the filament amounted to  $63\mu$ . During the first twelve minutes the rate of growth was  $0.75\mu$  per minute; during the next twenty-three minutes the growth was at the rate of nearly  $0.5\mu$  per minute; during the next forty-five minutes it was at the rate of  $0.4\mu$  per minute; and during the last forty minutes at the rate of a little over  $0.5\mu$  per minute.

These facts may be conveniently tabulated as follows:—

\* Air temperatures throughout, except where specially given as otherwise.

| Time.      | Length. | Interval. | Growth. | Approximate<br>rate per<br>minute. |
|------------|---------|-----------|---------|------------------------------------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ .                            |
| 10.10 A.M. | 81      | —         | —       | —                                  |
| 10.22 "    | 90      | 12        | 9       | 0.75                               |
| 10.45 "    | 102     | 23        | 12      | 0.5                                |
| 11.30 "    | 120     | 45        | 18      | 0.4                                |
| 12.10 P.M. | 144     | 40        | 24      | 0.6                                |

It is obvious that the growth is moderately rapid (the length would be doubled in about  $2\frac{1}{2}$  hours at same rate), but it seems to vary from time to time. An elongation of  $63 \mu$  in 120 minutes would give nearly  $0.3 \mu$  per minute at constant rate, whereas the rate varies considerably on either side of that.

These variations could not obviously be attributed to variations in temperature, for the thermometer was steadily rising the whole time, nor did I think they could be due to the measurements, for although the slight nutations do occasionally interfere with the strict accuracy of these, the disturbance can hardly be imagined to be so great as these variations imply, and repeated experience convinces me that this is not the explanation at all.

As already stated, the microscope was placed at a south window, under a bell-jar surrounded with black paper, and, except during the short periods necessary for drawing and recording, very little light could reach the object. The day was dull and rainy, but with somewhat brighter intervals. During the whole of the two hours the filament was describing the slight writhing movements which I regard as nutation curvatures, and the extent of which can be estimated by the drawings in fig. 13.

But another idea strikes one in connection with these measurements. If we take the germinating filaments, and draw them vertically to scale, on sectional paper, at any rate as regard the *lengths* attained during the various periods of growth, it is obvious that if their lengths are arranged as vertical lines (ordinates) on a base line divided into periods corresponding to the times (abscissæ) then the curve joining the tips of the filaments is *the curve of growth*, and clearly we may substitute mere vertical lines (ordinates) for the detailed drawing of the filaments themselves.

For instance, the curve of growth is got by straightening out the filaments of fig. 24, where all are drawn to scale at the indicated hours, and joining the tips—or, what amounts to the same thing, by joining the upper ends of ordinates of equal lengths erected on a base line.

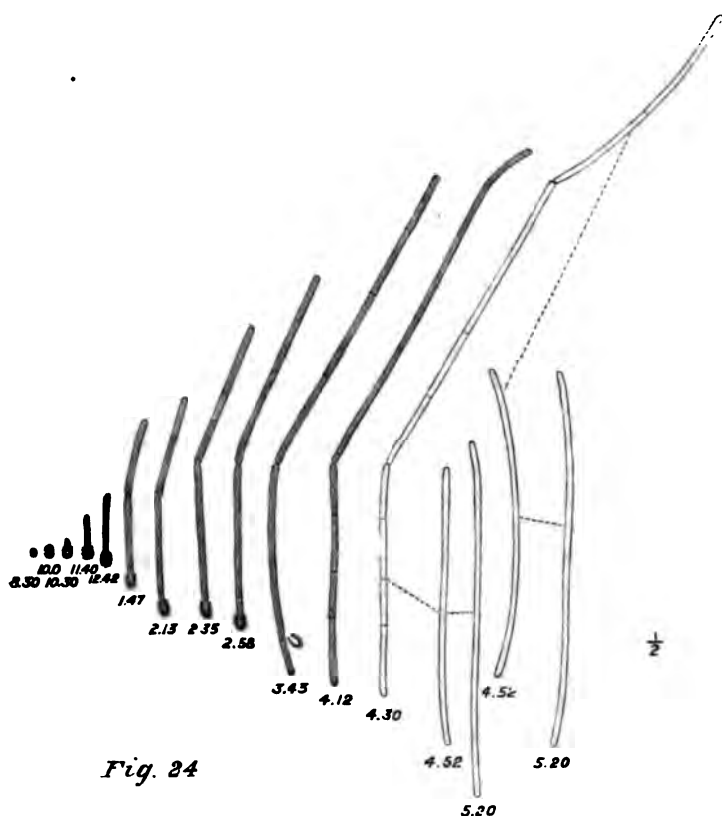


Fig. 24

In the further observations on growth, I availed myself largely of this idea and its consequences, with results of considerable interest and importance.

The following measurements of the rate of growth of the germinating filament (as contrasted with the longer older filament) were made. The spore was sown in 3 per cent. glucose solution (to which a little broth was added) shortly after 4 P.M.: the temperature was 20°—21° C. throughout.

From 5 P.M. to 6.5 P.M. the spore had swollen and elongated to 3  $\mu$ , and the measurements began at 8.10 P.M., when the germinal filament, still in the spore, measured 6  $\mu$ .

At 8.40 P.M. the young rodlet had elongated to 9  $\mu$ —i.e., it had grown 3  $\mu$  in thirty minutes. At 9.45 it had grown another 9  $\mu$ . From 9.45 to 11.0 P.M. it grew 12  $\mu$ . From 11.0 to 11.45 it had grown 6  $\mu$ .

The measurements were then discontinued till 10.35 next morning, when 45  $\mu$  additional had been added.

The total growth in length from 8.10 P.M., when the length recorded was  $6\ \mu$ , to 10.35 next morning, when the length was  $81\ \mu$ , therefore, amounted to  $75\ \mu$  in fourteen hours twenty-five minutes. This gives an average growth of about  $0.086\ \mu$  per minute, but the rate varied considerably from time to time. Thus during the first thirty minutes (from 8.10 to 8.40 P.M.) it was about  $0.33\ \mu$  per minute. During the next sixty-five minutes it averaged only about half that amount, a fact which puzzled me exceedingly, and which I thought might possibly be explained by the germinal rod being engaged in preparation for its first segmentation; for the first septum was visible shortly afterwards. But during the next period, of ninety-five minutes, the rate was apparently only about  $0.12\ \mu$  per minute; and during the next forty-five minutes, almost the same average rate was observed, whence I came to think the explanation temporarily entertained may have to be abandoned. Then came the break in the observations. During the night—11.45 P.M. to 10.35 A.M. = 650 minutes—the total growth was  $45\ \mu$ , i.e., about  $0.66\ \mu$  per minute on the average.

From the general slowness of growth of this specimen, I was more inclined to suppose that the medium (3 per cent. glucose solution) was less adapted for the needs of the organism than broth is, a conclusion fully borne out by tube-cultures, and subsequent experience.

Spores sown in broth at  $15^{\circ}\text{C}$ . at 3 P.M., had given rise to vigorously growing plants next morning, the long filaments of which were already breaking up as described on p. 279. One of these partial filaments was fixed under the micrometer scale, each division of which corresponded to  $3\ \mu$ , with the combination (E/4) used. The filament was quite straight, and its ends were covered by the 11th and 38th divisions respectively when the observations began. During the whole period the temperature was  $15^{\circ}$  to  $16^{\circ}\text{C}$ . and did not fall or rise outside those limits. Moreover, I kept the microscope at a north window, and by means of cardboard screens and by turning the mirror between the observations, prevented any access of direct light such as might possibly be suspected of inhibiting the growth.

At 10.18 the filament was straight and measured 27 divisions of the scale in length—i.e., it was  $81\ \mu$  long, and the following shows the growths at successive intervals.

| Time.      | Length. | Interval. | Growth. | Approximate average rate per minute. |
|------------|---------|-----------|---------|--------------------------------------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ .                              |
| 10.18 A.M. | 81.0    | —         | —       | —                                    |
| 10.36 "    | 85.5    | 18        | 4.5     | 0.25                                 |
| 10.49 "    | 98.5    | 13        | 3.0     | 0.25                                 |
| 11.2 "     | 94.5    | 13        | 6.0     | 0.5                                  |
| 11.15 "    | 96.0    | 13        | 1.5     | 0.12                                 |
| 11.30 "    | 98.5    | 15        | 2.5     | 0.17                                 |
| 11.42 "    | 100.5   | 12        | 2.0     | 0.17                                 |
| 12.6 P.M.  | 105.0   | 24        | 4.5     | 0.2                                  |
| 12.25 "    | 109.5   | 19        | 4.5     | 0.5                                  |
| 12.42 "    | 112.0   | 17        | 2.5     | 0.12                                 |
| 12.56 "    | 114.0   | 14        | 2.0     | 0.14                                 |
| 2.0 "      | 120.0   | 64        | 6.0     | 0.1                                  |
| 3.0 "      | 126.0   | 60        | 6.0     | 0.1                                  |
| 4.0 "      | 135.0   | 60        | 9.0     | 0.15                                 |

It may seem a remarkable coincidence that this partial filament was the same length as that measured previously, but the explanation is very simple, though I did not discover it till later: it is that the long filaments tend to begin breaking up into isolated segments of this length (each consisting of several cells) very early under certain circumstances.

I have given the measurements taken at 2, 3, and 4 P.M., but these are far less useful than those preceding them, for two reasons: (1) the intervals are too long, and (2) the filament underwent considerable curvatures from about 2 o'clock onwards, so that I cannot insist on the accuracy of the measurements so strongly as on those made previously.

If, now, we examine these results, it is clear that the filament grows more slowly at 15° C. than it does at 20° C., as was to be expected. It is also evident that considerable variations in the rate of growth occur during the whole period, and since I could not refer these to the action of any external causes, it seemed necessary to assume that we have here a case of periodicity due to internal causes of growth. Thus, there was an acceleration between 10.49 and 11.2, and another between 12.6 and 12.25. I have as yet failed to correlate these with any observed phenomenon, but it is clearly a question worth asking, whether the slower periods intervening were not perhaps the periods during which new septa were put into the filaments—i.e., period of cell-division. I was strongly inclined to think that is the case, though I had hitherto been unable to actually satisfy myself by directly observing the phenomenon. It is certain, however, that the filament at the beginning of the observations had fewer septa than at the end: for at 10.18 there were only 16 septa, whereas

at 4 o'clock there were certainly 23, and I believe more, perhaps 25 or 26, but those most recently formed are very difficult to see with the powers employed for measurements.

The whole period of growth under observation was (from 10.18 A.M. to 4 P.M.) six hours all but eighteen minutes, i.e., 342 minutes, and during that period the total elongation (from  $81\ \mu$  to  $135\ \mu$ ) amounted to  $54\ \mu$ , and taking the average length of the growing segments at  $5\ \mu$ , this would agree very well with the above, and, so far as it goes, is evidence in favour of the view I have supposed probable.

Having regard to the paucity of exact measurements after 12.56 in the above series, it is scarcely of value to note that the average rate of increment over the whole period, at  $15^{\circ}\text{C}$ ., seems to be about  $0.15\ \mu$  per minute; and the less useful since we have seen that there are such marked periods of slower and more rapid growth. Nevertheless, this would amount to a good deal in, say, sixty hours; if the germinal filament from a spore  $2\ \mu$  long continued to grow only at that rate, the filament produced would be  $600\ \mu$  long, and would be segmented into from 100 to 200 bacillar segments. But this is far less than actually occurs, as we shall see later.

In the following case the spore was sown, in dilute broth, a little after 10 A.M., and was put under observation by 11 A.M. The temperature was then  $16^{\circ}\text{C}$ .; but it fell to  $15^{\circ}$  by 4 P.M., and slowly down to  $12^{\circ}\text{C}$ . by 7 P.M. The spore was germinating at 12.10 P.M., when it measured  $3\ \mu$ .

| Time.      | Length. | Interval. | Growth. | Approximate rate per minute. |
|------------|---------|-----------|---------|------------------------------|
|            | $\mu$ . | h. m.     | $\mu$ . | $\mu$ .                      |
| 12.10 P.M. | 3.0     | —         | —       | —                            |
| 4.30 "     | 10.0    | 4 20      | 7.0     | 0.03                         |
| 5.40 "     | 13.5    | 1 10      | 3.5     | 0.05                         |
| 5.54 "     | 15.0    | 0 14      | 2.5     | 0.18                         |
| 6.12 "     | 17.0    | 0 18      | 2.0     | 0.11                         |
| 6.27 "     | 18.0    | 0 15      | 1.0     | 0.07                         |
| 6.45 "     | 19.0    | 0 18      | 1.0     | 0.06                         |
| 7.5 "      | 22.5    | 0 20      | 3.5     | 0.17                         |

This gives a total growth of  $19.5\ \mu$  in six hours and fifty-five minutes, with an average growth of  $0.09\ \mu$  per minute if the rate were constant.

I was unable to distinguish any trace of a second septum up to 6.45, but at 7.5 the rod showed a perfectly sharp median septum, and two extremely faint secondary septa—one on each side.

I thought there could be little doubt that the sharp fall in the rate of growth at 6.27 and 6.45 was associated with the process of cell.

division which initiated these new septa, because the fall in temperature seemed probably too gradual and slow to account for it. No doubt these septa were already present at 6.45, and the increased rate of growth followed on their inception, but, with the power employed, they would be invisible for some time, as experience had shown me in other cases.

The following series were traced on a segment broken off from one of the filaments of the same culture as the last, and is therefore practically a continuation of that series into the night. It clearly bears out the same conclusion, that the period of cell-division entails more or less cessation of growth. In this case the temperature was 12° C. throughout the whole period during which the rod was kept under observation, and the whole growth took place in the dark, consequently the variations noticed cannot have been brought about by recognisable changes in the environment, but must be referred to internal causes.

| Time.    | Length.  | Interval.        | Growth.  | Approximate rate per minute. |
|----------|----------|------------------|----------|------------------------------|
|          | $\mu$ .  | h. m.            | $\mu$ .  | $\mu$ .                      |
| 7.8 P.M. | 5.0 (rod | with no trace of | septum). | —                            |
| 7.34 "   | 14.0     | 0 26             | 7.0      | 0.27                         |
| 8.39 "   | 22.5     | 1 5              | 6.5      | 0.10                         |
| 9.10 "   | 25.5     | 0 31             | 3.0      | 0.09                         |
| 9.28 "   | 27.0     | 0 18             | 1.5      | 0.08                         |
| 9.55 "   | 30.0     | 0 27             | 3.0      | 0.11                         |
| 10.23 "  | 34.0     | 0 28             | 4.0      | 0.14                         |
| 10.48 "  | 38.0     | 0 25             | 4.0      | 0.16                         |

In this case no septum was visible at 7.8 P.M., but there was one at 7.34 P.M., and the rod consisted of two cells. At 8.39 to 9.28 we have another period of slower growth, and during this interval each cell put in another septum; then it increased again, up to 10.48 P.M., when the observations ceased for the night.

At 8.30 on the following morning the filament measured 144  $\mu$ , having grown 106  $\mu$  during the nine hours and forty-two minutes, which would give 0.18  $\mu$  per minute as the approximate average if the rate were constant. This seems a somewhat high rate for the temperature (12° C.), and suggests a question which I afterwards tested; the question is, does the average rate of growth, other things being equal, increase during the dark hours of the night?

The following series, however, drew my attention to some questions of temperature more definitely than hitherto, and led, as we shall see, to some interesting results in this connection.

A spore, sown soon after 10 A.M. in dilute broth, was kept entirely

in the dark throughout the whole period of the experiment, except at the short intervals (one to two minutes each) necessary for recording the growth.

The temperature at 10—11 A.M. was 16° C., but it rose to 20—21° C. at 2 P.M., and remained at that until about 7 P.M., when it fell until it was again 16° C. at 8.30 P.M., and stayed so through the night.

The spore germinated vigorously, and the germinal rodlet was 10  $\mu$  long by 2 P.M. The following table shows its further behaviour:—

| Time.   | Length.   | Interval. | Growth. | Approximate rate per minute. |
|---------|-----------|-----------|---------|------------------------------|
|         | $\mu$ .   | h. m.     | $\mu$ . | $\mu$ .                      |
| 11 A.M. | 2 (spore) | —         | —       | —                            |
| 2 P.M.  | 10·0      | 3 0       | 8·0     | 0·44                         |
| 4.45 "  | 57·5      | 2 45      | 47·5    | 0·3                          |
| 5.25 "  | 77·5      | 0 40      | 20·0    | 0·5                          |
| 5.42 "  | 102·5     | 0 17      | 25·0    | 1·5                          |
| 5.52 "  | 115·0     | 0 10      | 12·5    | 1·25                         |
| 6.10 "  | 125·0     | 0 18      | 10·0    | 0·55                         |
| 6.25 "  | 145·0     | 0 15      | 10·0    | 0·7                          |
| 6.43 "  | 160·0     | 0 18      | 15·0    | 0·8                          |
| 7.2 "   | 180·0     | 0 19      | 20·0    | 1·0                          |
| 7.15 "  | 190·0     | 0 13      | 10·0    | 0·8                          |
| 7.36 "  | 210·0     | 0 21      | 20·0    | 1·0                          |
| 8.36 "  | 225·0     | 0 60      | 15·0    | 0·25                         |
| 9.10 "  | 240·0     | 0 34      | 15·0    | 0·44                         |
| 9.27 "  | 250·0     | 0 17      | 10·0    | 0·6                          |

On scrutinising this table and drawing the curve representing the increments, it is evident that we have the effects of the higher temperature expressing themselves in conjunction with those (if there are any) due to absence of light.

The characters of the culture itself were all those of a superbly vigorous organism, and the crop of spores developed at the end of its growth—the spores were ripe in 72 hours from sowing—was a remarkably fine one.

As we see, in the period of growth recorded from 2 P.M. to 9.27 P.M., the rodlet elongated from 10 to 240  $\mu$ . In other words, it grew 230  $\mu$ , which is 115 times the length of the spore, in 7½ hours. This would give an average growth of about 0·5  $\mu$  per minute throughout the period referred to.

On comparing these results with what were obtained during growth at the same temperature in ordinary daylight, I could not see how to escape the conclusion that the rapid rise of the curve is chiefly due to the absence of the inhibitory effect of the light.\*

\* Unless—a question which arose later—the temperature *inside* the culture-chamber is very different from that recorded by the thermometer standing, as here, outside the system.

Moreover, the same conclusion forced itself on one if other curves are compared: the general slope in each case is undoubtedly largely due to the lower temperature, but the decided rise in some cases about 6 p.m., when the daylight failed, took place *in spite of the falling temperature*, and the generally steeper ascent of others at this low temperature might be due to the cessation of light-action—at least I could see no other way of explaining it at the time.

It is clear from the foregoing results that the growth of the rods and filaments of this bacillus *can* be measured by the methods devised, and that the undertaking presents no particular difficulties so far, beyond those incident to all close and patient microscopic investigation.

But, beyond the fact that growth occurs at various rates, and can be thus measured, the results thus far give us far too little information to be of the value I anticipated from the application of the method. They simply raise a number of questions as to the action of various factors in influencing the course of growth and inducing the variations in the rate of growth which undoubtedly occur.

Such factors are (1) internal factors, such as the age of the filament, the process of cell-division, and possibly the vigour of the spore itself; and (2) external factors, such as temperature, light of various kinds, and the food-materials, &c.

#### *Growth and Cell-division.*

I made the following observations with a view to obtain more information concerning the question raised as regards the connection between growth and cell-division. The chief difficulty connected with them was, as might be expected, that very high powers have necessarily to be employed, and all the troubles of thin cover-slips, minute and not too deep drops, and so forth, arise.

I selected the tip of a filament which had been growing vigorously all the afternoon and evening, and which was in all about  $600\ \mu$  long, and traced its behaviour under the  $1/12$ th oil immersion. The piece I chose was the terminal portion from the tip to the first visible septum. Its length was measured as exactly as possible, and when the septum was under the division 0 of the scale the tip was exactly under the 14th division.

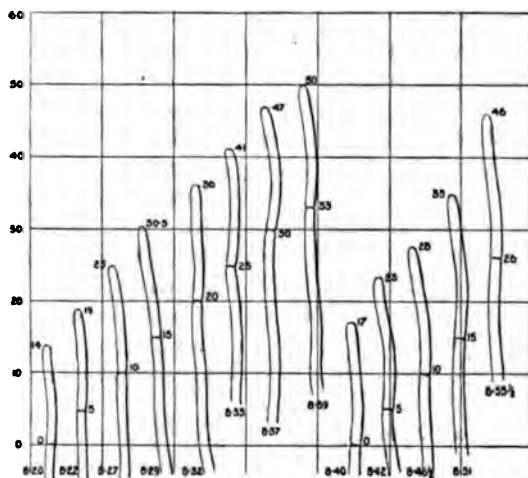
I had previously determined that six divisions on the scale used, each representing  $4.5\ \mu$  with my ordinary measuring combination, are equal to fifteen divisions by this power, and therefore I was measuring nearly  $2\ \mu$  per division (more exactly =  $1.82\ \mu$ ).

At 8.20 p.m. the segment in question measured (under the  $1/12$ th immersion) fourteen divisions, i.e.,  $14 \times 1.82 = 25.48\ \mu$ , and I started

the observations. The scale consisted of fifty divisions, and I started with the septum at 0 and the tip at 14. The following table gives the positions of this septum and tip at successive intervals as (1) the growth of the terminal segment carried tip and septum further apart, and (2) the elongation of the rest of the filament as a whole pushed the entire segment forward over the scale.

It will be noticed that to do this I observed the successive periods during which the filament pushed the septum over five divisions, as I found that the easiest plan, and then recorded the position of the tip at the time (see fig. 26).

FIG. 26.



| Time.     | Division of<br>scale over<br>septum. | Division of<br>scale<br>over tip. | Growth<br>of<br>segment | Interval. | Rate. |
|-----------|--------------------------------------|-----------------------------------|-------------------------|-----------|-------|
| 8.20 P.M. | 0                                    | 14.0                              | —                       | mins.     | —     |
| 8.22 "    | 5                                    | 19.0                              | —                       | —         | —     |
| 8.27 "    | 10                                   | 25.0                              | 1.82                    | 7         | 0.26  |
| 8.29 "    | 15                                   | 30.3                              | —                       | —         | —     |
| 8.32 "    | 20                                   | 36.0                              | 1.82                    | 5         | 0.36  |
| 8.35 "    | 25                                   | 41.0                              | —                       | —         | —     |
| 8.37 "    | 30                                   | 47.0                              | 1.82                    | 5         | 0.36  |
| 8.40 "    | 0*                                   | 17.0                              | —                       | —         | —     |
| 8.42 "    | 5                                    | 23.0                              | 1.82                    | 5         | 0.36  |
| 8.46½ "   | 10                                   | 28.0                              | 1.82                    | 4½        | 0.4   |
| 8.51 "    | 15                                   | 35.0                              | 3.64                    | 4½        | 0.8   |
| 8.55½ "   | 26                                   | 46.0                              | —                       | —         | —     |

\* i.e., I brought the septum back again so as to lie under the division 0 on the scale.

Here we see the segment elongated from fourteen divisions to twenty divisions, *i.e.*, it grew six divisions ( $= 6 \times 1.82 = 10.92 \mu$ ) in the interval from 8.20 to 8.51—*i.e.*, in thirty-one minutes—giving an average rate of growth of about  $0.35 \mu$  per minute, though the rate of growth varies from time to time.

Now the whole filament was 600 divisions long ( $= 1,092 \mu$ ), and consisted of at least forty such segments,\* and if each of them was growing at anything like this rate, no wonder the filament pushed this segment forward so quickly, for it would be elongating as a whole at the rate of  $14 \mu$  per minute.

To gain further information in this connection, I exchanged the objective for the combination I usually employ for measuring, and measured the growth of about a third of the whole filament (including the part here concerned) during the seven minutes from 9 to 9.7 P.M., *i.e.*, beginning  $4\frac{1}{2}$  minutes after the last measurement.

At 9 P.M. the piece observed was  $210 \mu$  long, and at 9.7 P.M. it had elongated to  $250 \mu$ , giving a growth of  $40 \mu$ , which is at the rate of  $5.7 \mu$  per minute. If this was going on through the rest of the length, the filament as a whole would be growing at a rate considerably in excess of my estimate.

Now if we look at the distance through which the measured segment was pushed during the thirty-one minutes' period given above, we find it amounts to forty-five divisions ( $= 45 \times 1.82 = 81.90 \mu$ ), so that even such numbers as I have proposed need not seem extravagant, and indeed I have reason to know they are much below the real ones in many cases.

One of the most interesting cases of rapid growth I have seen is the following.

Spores sown in normal gelatine at  $22^\circ \text{C}$ . had germinated out to filaments  $80$ — $100 \mu$  long in  $5\frac{1}{2}$  hours in the dark, and were then put into the dark Sachs' box† at  $28^\circ \text{C}$ . The temperature was then slowly raised, so that in two hours it had risen to  $34^\circ \text{C}$ ., half an hour later to  $38^\circ \text{C}$ ., and in the next quarter of an hour to  $39^\circ \text{C}$ . By chance I happened now to catch a broken-off segment which was growing at the maximum rate that precedes death at these high temperatures. The measurements were as follows:—

\* Almost certainly more than 40, but I could not determine accurately because the last formed septa are not sufficiently distinct.

† See p. 394 for description of this box and the method of using it.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temperature.         |
|-----------|---------|-----------|---------|---------|----------------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . |
| 5.12 P.M. | 139.5   | —         | —       | —       | 39.0                 |
| 5.14 "    | 153.0   | 2         | 13.5    | 6.7     | 39.1                 |
| 5.15½ "   | 175.5   | 1½        | 22.5    | 15.0    | 39.25                |
| 5.17 "    | 198.0   | 1½        | 22.5    | 15.0    | 39.2                 |

Here we see growth going on at the enormous rate, hardly measurable, of 15  $\mu$  per minute. In such cases the growing tip is seen moving almost like an *Oscillatoria*.

Then the filament suddenly contracted and broke up, and in a short time presented the granular appearance of dead cells.

The following measurements under the 1/12th immersion were made on a germinal filament which had emerged from the spore during the night. The culture in broth was exposed to the daylight the whole time,\* but the sun was obscured by haze and clouds. The temperature was rising slowly from 18 $^{\circ}$  to 21 $^{\circ}$  C., also during the whole period observed.

Each division of the micrometer scale was in this case again equal to 1.82  $\mu$ , and the measurements were made as 1, 2, 3, or 4 divisions were travelled over.

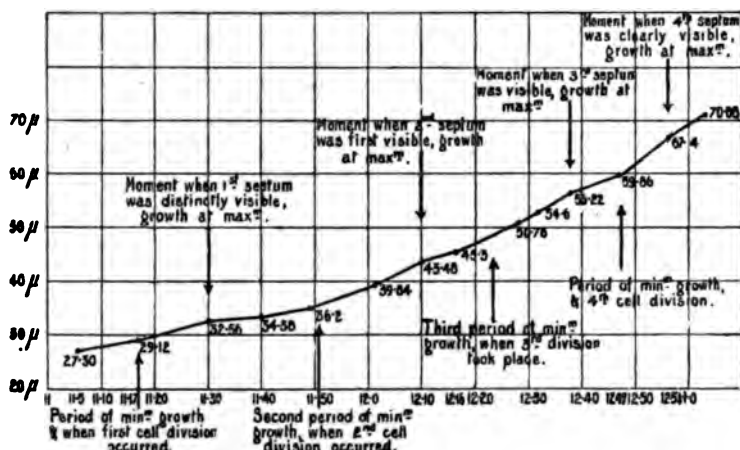
| Time.     | Length. | Interval. | Growth. | Rate.   | Temperature of air.  |
|-----------|---------|-----------|---------|---------|----------------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . |
| 11.5 A.M. | 27.30   | —         | —       | —       | 18.0                 |
| 11.17 "   | 29.12   | 12        | 1.82    | 0.15    | 18.0                 |
| 11.30 "   | 32.56   | 13        | 3.64    | 0.28    | 18.0                 |
| 11.40 "   | 34.38   | 10        | 1.82    | 0.18    | 18.5                 |
| 11.49 "   | 36.20   | 9         | 1.82    | 0.20    | 18.5                 |
| 12.1 P.M. | 39.84   | 20        | 3.64    | 0.18    | 18.5                 |
| 12.10 "   | 43.48   | 9         | 3.64    | 0.40    | 19.0                 |
| 12.16 "   | 45.30   | 6         | 1.82    | 0.30    | 19.0                 |
| 12.28 "   | 50.76   | 12        | 5.46    | 0.42    | 19.0                 |
| 12.38 "   | 56.22   | 10        | 5.46    | 0.54    | 19.5                 |
| 12.47 "   | 59.86   | 9         | 3.64    | 0.18    | 20.0                 |
| 12.56 "   | 67.14   | 9         | 7.28    | 0.81    | 20.5                 |
| 1.2 "     | 70.88   | 6         | 3.64    | 0.60    | 21.0                 |
| 1.5 "     | 74.52   | 3         | 3.64    | 1.21    | 21.0                 |

Here we have a total growth of the young filament amounting to 47.22  $\mu$  in 120 minutes, which would give an average rate of nearly

\* It should be noted that in these high-power observations a bright illumination has of course to be employed.

0.4  $\mu$  per minute if constant. But obviously the rate was not constant, as the table and curve show (see fig. 27).

FIG. 27.



I made the following observations in addition to see if the variations were connected with cell-division as suspected. Up to 11.17 no septum was really visible\* in the young filament, but a distinct median one was seen at 11.30 dividing the filament into a proximal and a distal half. By 12.10 a second septum was clearly visible bisecting the distal half, but none was as yet visible in the proximal half to which the spore membrane still clung. At 12.38 the proximal segment was also divided by a visible median septum. At 12.47 I measured both the primary segments and found the proximal, recently divided, one shorter than the distal one, in the ratio of 15 to 18; that is to say, the whole filament measured 59.86  $\mu$ , as seen, but that part of it to the proximal side of the first septum was only 27  $\mu$ , that to the distal 33  $\mu$  in length; so that already the symmetry of the filament was disturbed, and further measurements confirmed this.

At 12.56 the proximal segment had one septum, now very distinct, and measured 30.5  $\mu$ , whereas the distal one was by this time provided with two visible septa, and measured nearly 37  $\mu$ ; at 1.5 P.M. the proximal one still had but one septum visible, and measured 32.5  $\mu$ , whereas the distal one, with its two visible septa, measured 42.5  $\mu$ .

Hence we see the two primary segments resulting from the first division of the germinal filament grow and (so far as visible segments

\* Probably thin septa were present, but they were not visible in the living and rapidly growing filament.

show) divide at different rates from the first. I am disposed to regard the slower growth of the proximal segment, in part at any rate, to its being more especially concerned with the absorption of food-materials from the spore; though the fact that it is still behind-hand, even after escaping from the spore-membrane, may indicate a deeper meaning—possibly that differences between basal and apical regions are more strongly defined in these organisms than we suppose.

But another point must be considered before the curve can be understood, and for this purpose it seems necessary to introduce a simple nomenclature for the divisions and segments.

We may term the first septum, which divides the whole germinal filament into its first two segments, the primary septum; thus the primary septum was first *visible* at 11.30, dividing the filament into a proximal and a distal primary segment. At 12.10 the longer *distal* primary segment showed a further division by a *secondary* septum into two *secondary* segments; but the corresponding secondary septum in the *proximal* primary segment was not visible until 12.38.

At 12.56 the distal secondary segment of the primary distal segment had a *tertiary* septum plainly visible, and the filament as a whole, therefore, consisted at this hour of five visible portions, two belonging to the primary proximal segment, and three belonging to the primary distal segment.

On turning to the curve of growth (fig. 27), it may now be possible to understand its principal features if we first accept as a fact that the period when a septum is first *distinctly visible* in these brilliant living cells is some time *after the moment of actual cell-division*. This, I think, must be accepted, because I find stained preparations of such filaments show many more septa to be actually present than can be seen in the living filaments, owing to the extremely high refrangibility of the protoplasm obscuring the view of the most recently formed and still tenuous walls.

In the curve referred to, as I understand it, its general form, with a higher and higher rate of ascent as time goes on, is due to the total increasing elongation of all the segments simultaneously—aided in this case by the slight continuous rise of temperature.

But, although the general sweep of the curve is such, there are clear indications of smaller curves, convex also to the abscissæ, on which points of maximum rate of growth are seen at 11.30, 12.10, 12.38, and 12.56, and points of minimum rate in each case about mid-way between these times.

Now, it was at just these periods—the intervals between which are approximately 40, 30, and 20 minutes respectively—that the new cell-walls were first perceptible, and it seems almost certain that the intervening periods of slowest rate of growth, viz., about 11.15, 11.50, 12.20, and 12.40 respectively, were the approximate moments

of insertion of the septa, and, consequently, moments when elongation would be going on with least rapidity, because the cell-contents were too busy with the act of division to be then growing at quickest rates.

In any case, it is obvious that the growth of even these young filaments is a complicated phenomenon, and that the following points at least have to be regarded in considering it.

(1) There is the total elongation of the filament as a whole, and (2) the growth of the individual segments or cells; (3) the growth of any one segment or cell is not necessarily going on at exactly the same rate as a neighbouring one: this depends on the position and on the age of the cell concerned; (4) there is evidence to show that the rate of growth of any cell or segment, and consequently that of the filament, as a whole, is profoundly affected by several factors, such as temperature, the nature of the food-medium, the presence of other filaments which may aid the given one (at certain stages) by increasing the peptonisation in the immediate neighbourhood—though we must believe that in other stages these neighbouring filaments retard it by acting as competitors for the available food; and, lastly, possibly light, some rays of which (perhaps blue) may retard the growth, while others (possibly the red and infra-red) may accelerate it.

The following observations with the 1/12th immersion were made on segments of a two-days' growth in broth, with a trace of gelatine, in bright light—not direct sunlight—at 17° to 18° C.

A terminal segment showing three distinct septa, but already composed of at least eight cells—the other four (secondary) septa being very difficult to observe—was 54·6  $\mu$  long at 9.5 A.M. The growths are shown in the following table, and, as before, we see that considerable irregularity is observable in the rate from time to time:—

| Time.    | Length. | Interval. | Growth. | Rate.   |
|----------|---------|-----------|---------|---------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . |
| 9.5 A.M. | 54·6    | —         | —       | —       |
| 9.12 "   | 56·42   | 7         | 1·82    | 0·26    |
| 9.23 "   | 58·24   | 11        | 1·82    | 0·17    |
| 9.31 "   | 60·06   | 8         | 1·82    | 0·22    |
| 9.38 "   | 60·97   | 7         | 0·91    | 0·13    |
| 9.40 "   | 61·88   | 2         | 0·91    | 0·45    |

The segment was being pushed forward by the rest of the filament at the rate of about 1  $\mu$  per minute—36·4  $\mu$  in 34 minutes actually measured—and was slightly undulating the whole time. In fact the movement forwards, a scarcely perceptible series of jerks, reminded one of the movements of the hands of a watch.

At 9.45 another segment, also a terminal one, was selected for observation, and behaved as follows:—

| Time.     | Length. | Interval. | Growth. | Rate.   |
|-----------|---------|-----------|---------|---------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . |
| 9.45 A.M. | 18.2    | —         | —       | —       |
| 9.50 "    | 18.11   | 5         | 0.91    | 0.18    |
| 10.5 "    | 20.02   | 10        | 0.91    | 0.09    |
| 10.20 "   | 20.93   | 15        | 0.91    | 0.06    |
| 10.50 "   | 33.67   | 30        | 12.74   | 0.42    |

Here, again, the extraordinarily irregular growth comes out clearly, looking as if the segment, having gradually slowed down its rate of growth to a minimum from 9.45 to 10.20, suddenly began to grow at a rapid rate again.

The following measurements were made on a terminal segment, of the same culture, measuring 18.2  $\mu$ , and showing one distinct septum only, though each cell was almost certainly divided by a median septum during the period.

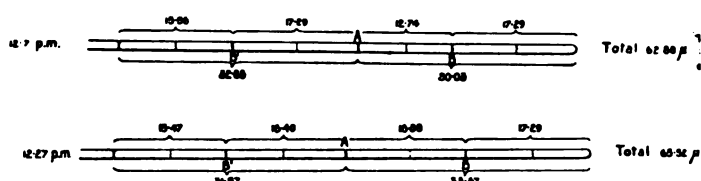
| Time.     | Length. | Interval. | Growth. | Rate.   |
|-----------|---------|-----------|---------|---------|
|           | $\mu$   | mins.     | $\mu$ . | $\mu$ . |
| 11.0 A.M. | 18.2    | —         | —       | —       |
| 11.20 "   | 18.2    | 20        | 0       | 0       |
| 11.35 "   | 21.02   | 15        | 1.82    | 0.12    |
| 11.55 "   | 20.02   | 20        | 0       | 0       |

At the end of the period I assured myself that the segment contained one very distinct septum dividing it into two parts, or secondary segments; the apical one measuring not quite 11  $\mu$  (my numbers give 10.92), and the other a little over 9  $\mu$  (9.10), and each of these had an extremely faint median septum in it. According to this, the segment 20  $\mu$  long consisted of four cells each, more or less, 5  $\mu$  long, and we must infer that they were growing at slightly different rates part of the time.

Another terminal segment of the same culture was watched under the 1/12th immersion. It measured 62.88  $\mu$  at 12.7 (P.M.) and consisted of eight visible cells, and probably each of these with a septum. As near as I could measure, the primary septa, which were very distinct, stood as follows: One (which I will call A) stood at 30.03  $\mu$  from the apex, dividing the whole segment selected into a (fig. 28) distal (apical) moiety measuring almost exactly 30  $\mu$ , and a proximal

moiety measuring  $31.85 \mu$ . Each of these portions was again sub-divided by a septum; the distal one, by a septum B, into a distal moiety just over  $17 \mu$  in length, and a proximal moiety not quite  $13 \mu$  long; the proximal one, by a septum B', into a distal moiety measuring just over  $17 \mu$ , and a proximal moiety slightly more than  $15.5 \mu$ . Close observation showed that each of these was again sub-divided by a faint, approximately median septum, into two cells.

FIG. 28.



Now it is perfectly obvious from this that the various subsidiary segments or cells, measured at any period, are not of exactly the same length.

At 12.27 I again measured, as exactly as possible, the relative positions on the scale of the above septa to see if I could determine the relative rates of growth of the various segments in the intervening twenty minutes.

The septum A had moved backwards nearly  $3 \mu$  on the scale, and the apex and base of the whole segment were driven apart about this distance. This suggests that the growth (intercalary) had occurred chiefly in the distal part of the original segment, and such was the case.

The distance between the septum B and the apex was not measurably altered, i.e., the part to the distal side still measured  $17.29 \mu$ ; but the distance between B and A had increased from  $12.74$  to  $16.38 \mu$ . On the proximal side of the septum A no appreciable changes had occurred. It is true, my measurements, as multiplied out, are a trifle different from those first made, but the differences may be neglected, as of course I could not measure to the decimals concerned.

What does come out, and very clearly, is that during the interval named the growth was entirely in one only of the four cells, and not in all of them. It is obvious that such phenomena complicate the question of the growth of the entire filament still more, and no doubt some of the minute variations observed are due to such events as these.

I made the following measurements under the  $1/20$ th immersion, on a terminal segment  $36 \mu$  long, in the sister culture to the last one,

standing side by side with it. The temperature was rather low, 15° C., and the observations soon came to an end owing to the close-focussing objective cracking the very thin cover slip. I made many attempts to measure growths for longer periods under this lens, but the failures were so frequent owing to the extremely thin cover slips and hanging drops necessary for so high a power, and the difficulties of illumination, that I had to abandon them.

The value of each division of the micrometer scale was found to be from 1·20  $\mu$  to 1·25  $\mu$ , but here again I found it hopeless to attain to greater exactitude of measurement. However, the following attempt is at least interesting.

| Time.     | Length. | Interval. | Growth. | Rate.   |
|-----------|---------|-----------|---------|---------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . |
| 9.17 A.M. | 36·0    | —         | —       | —       |
| 9.19½ „   | 37·8    | 2½        | 1·8     | 0·7     |
| 9.22 „    | 38·4    | 2¼        | 0·6     | 0·24    |

On comparing the figures with other measurements at the same temperatures, moreover, they agree very well; so that it may yet be possible to carry out measurements with this power.

It should be noticed—as a point of importance in what follows—that these minute variations observed with the highest powers are not traceable (and probably neutralise one another along the filaments) when longer stretches are measured with the lower powers.

After considerably greater experience with these curves, I am able to sum up the meaning of these experiments more clearly.

1. They give evidence that the growth of the filament as a whole is intercalary, and due to increase in length and division of all its cells, along the entire course of the filament.

2. The different rates in the general growth observed are due partly to differences in temperature, partly to differences in age of the portions observed, partly to differences in the food-medium in which the organism is growing, and partly to other causes.

3. The small variations in rate of growth, especially those traced under high powers, are due partly to small and unrecorded variations in temperature, *e.g.*, cooling of the thin cover-slip when the bell-jar was lifted (as in the experiment on p. 306), and partly to the causes assigned on p. 302, namely, pauses during the intercalation of the new segment walls, and, no doubt, to some extent, to curvatures in the filaments, and want of practice on my part in recording the observations so accurately as I learned to do later on.

*Comparative Measurements.*

The foregoing results led to the attempts—now to be described—to grow two filaments side by side, one in the light and the other in the dark, to see if the action of light could be detected by any change in the growth curve.

Before passing to these experiments, I obtained satisfactory evidence that two cultures, side by side and under the *same* conditions, behave similarly.\*

On February 18th I started the following comparative cultures, to see how far I could test the action of daylight rendered so probable by some of the foregoing results.

Spores were sown in broth at 9 A.M. in two hanging drop cultures, each with a layer of water at the bottom to prevent rapid changes of temperature or drying up. The two cultures were then put under a dark bell-jar, covered with foil and brown paper, at 18° C., rising to 21° C., close to the south window where I intended to start the experiment. The cultures were left thus so that the spores should have time to germinate out normally, which they began to do about noon; they were left undisturbed till 2 P.M.

Meanwhile, I had selected two similar bell-jars and placed these a the window, each over a microscope provided with a thermometer; one bell-jar was darkened with tin foil and brown paper, the other not. The thermometers were examined, and readings taken from time to time. The differences observed at first, and due to the shading action of the covering, began to get less and less as the bell-jars, table, and microscopes, were warmed by the sun, which was bright and hot; and by noon the temperatures were very nearly the same in each jar.

At 12.30 the thermometer under the light jar indicated a temperature of 25.5° C.; that under the darkened one = 24.6° C.,† and this difference of nearly 1° C. was maintained till about 1.30, the readings being as follows:—

|           | 12.30 P.M. | 12.45 P.M. | 1.0 P.M. | 1.15 P.M. | 1.30 P.M. |
|-----------|------------|------------|----------|-----------|-----------|
|           | °C.        | °C.        | °C.      | °C.       | °C.       |
| Dark ...  | 25.5       | 26.0       | 27.0     | 27.5      | 28.0      |
| Light ... | 24.6       | 25.5       | 26.0     | 27.0      | 27.5      |

About 2 P.M. the sun began to sink behind trees and the roof of a building facing the window, and now it was possible to use the bright light from the sky without danger of direct insolation.

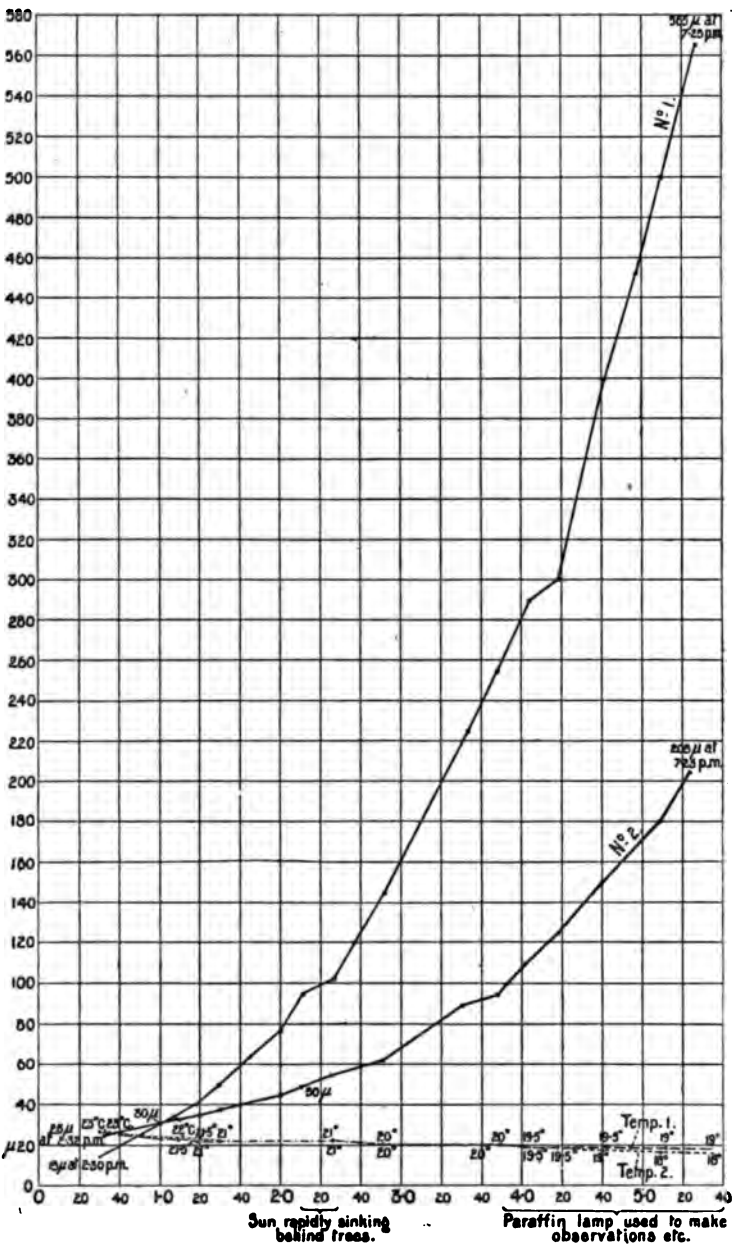
\* Better proofs of this were obtained later, however, and are given at pp. 359—361.

† These are all air temperatures, unless the contrary is specially stated.

At 2.20 p.m. the thermometer in both bell-jars stood at 23° C., and, as the curve of temperature shows, they were so nearly equal throughout the experiment that I had little hesitation in concluding that the cultures did not vary much from the same temperature at any time; because I thought it could hardly be imagined that the hanging drops in these damp cells rapidly vary with the temperature of the environment, and still less so the filaments growing in them.

At 2.30 the observations were begun by noting the temperature, and measuring the rod in each case. As the following tabular record shows, the difference in time between the two notes rarely exceeded a couple of minutes, whence the two series are closely comparable in all respects.

It was not until much later that the question arose—or, rather, acquired the great importance I now attach to it—how far the culture in the light could avail itself of or be affected by the infra-red rays reflected from the mirrors, and so complicate the matter of temperature effects in these experiments. This matter is of pregnant importance in its bearing on all physiological experiments of this kind, however.



| Culture in dark. |         |           |         |                              |              | Culture in light. |         |           |         |                              |              |
|------------------|---------|-----------|---------|------------------------------|--------------|-------------------|---------|-----------|---------|------------------------------|--------------|
| Time.            | Length. | Interval. | Growth. | Approximate rate per minute. | Temperature. | Time.             | Length. | Interval. | Growth. | Approximate rate per minute. | Temperature. |
| P.M.             | μ.      | mins.     | μ.      | μ.                           | °C.          | P.M.              | μ.      | mins.     | μ.      | μ.                           | °C.          |
| 2.30             | 15.0    | —         | —       | —                            | 23.0         | 2.32              | 25.0    | —         | —       | —                            | 23.0         |
| 3.8              | 35.0    | 88        | 20      | 0.5                          | 21.5         | 3.10              | 32.5    | 88        | 7.5     | 0.2                          | 22.0         |
| 3.19             | 40.0    | 11        | 6       | 0.45                         | 21.0         | 3.20              | 35.0    | 10        | 2.5     | 0.25                         | 21.5         |
| 3.29             | 50.0    | 10        | 10      | 1.0                          | "            | 3.30              | 38.0    | 10        | 3.0     | 0.3                          | 21.0         |
| 3.45             | 65.0    | 16        | 15      | 0.95                         | "            | 3.40              | 40.0    | 10        | 2.0     | 0.2                          | "            |
| 4.0              | 78.0    | 15        | 13      | 0.86                         | "            | 4.0               | 44.0    | 20        | 4.0     | 0.2                          | "            |
| 4.11             | 96.0    | 11        | 18      | 1.6                          | "            | 4.10              | 49.0    | 10        | 5.0     | 0.5                          | "            |
| 4.26             | 102.0   | 15        | 6       | 0.4                          | "            | 4.25              | 55.0    | 15        | 6.0     | 0.4                          | "            |
| 4.52             | 145.0   | 26        | 43      | 1.6                          | 20.0         | 4.50              | 62.0    | 25        | 7.0     | 0.28                         | 20.0         |
| 5.32             | 225.0   | 40        | 80      | 2.0                          | "            | 5.30              | 90.0    | 40        | 28.0    | 0.7                          | "            |
| 5.48             | 255.0   | 16        | 30      | 1.87                         | "            | 5.47              | 95.0    | 17        | 5.0     | 0.3                          | "            |
| 6.3              | 290.0   | 15        | 35      | 2.3                          | 19.5         | 6.1               | 110.0   | 14        | 15.0    | 1.0                          | 19.5         |
| 6.19             | 300.0   | 16        | 10      | 0.6                          | "            | 6.18              | 125.0   | 17        | 15.0    | 0.88                         | "            |
| 6.41             | 400.0   | 22        | 100     | 4.5                          | "            | 6.40              | 150.0   | 22        | 25.0    | 1.1                          | 19.0         |
| 7.10             | 500.0   | 29        | 100     | 3.4                          | 19.0         | 7.8               | 180.0   | 28        | 30.0    | 1.0                          | 18.0         |
| 7.25             | 565.0   | 15        | 65      | 4.3                          | "            | 7.23              | 205.0   | 15        | 25.0    | 1.66                         | 18.0         |
| 10.45            | —       | —         | —       | —                            | 18.0         | 10.45             | —       | —         | —       | —                            | 18.0         |

Curves 1 and 2.

\* Sun sinking fast behind trees, and light beginning to fade.

† For these (5.47 and 5.48 P.M.) and succeeding observations I had to use artificial light; I employed a paraffin lamp, about 2 feet from microscope, but the time occupied in making the observations was probably too short for perceptible effects.

The doubling periods\* calculated on the curves were as follows :—

1. Dark culture]

|             |               |        |                    | Approximate<br>(air) temps. |
|-------------|---------------|--------|--------------------|-----------------------------|
| 15—30 $\mu$ | = 2.30 to 3   | P.M. = | 30 minutes at 23.0 | —21.75° C.                  |
| 30—60       | = 3.0 „ 3.40  | = 40   | „ „                | 21.75—21                    |
| 60—120      | = 3.40 „ 4.37 | = 57   | „ „                | 21 —20.5                    |
| 120—240     | = 4.37 „ 5.40 | = 63   | „ „                | 20.5 —20                    |
| 240—480     | = 5.40 „ 7.4  | = 104  | „ „                | 20 —19                      |

Now my constant temperature cultures in broth in the dark at 23—21.75° run from 32 minutes at 23—24° C. to 63 (at 20—21°) and at 18—19° C. the extremes are 67—78 minutes; the range is a wide one—*i.e.*, 32—78—but it may be regarded as including the above cases, or very nearly so, and perhaps, therefore, the air-temperatures given did not depart far from those of the culture cells.

2. The culture in the light for the same date has the following doubling periods :—

|             |             |                     |         | Approximate<br>(air) temps. |
|-------------|-------------|---------------------|---------|-----------------------------|
| 25—50 $\mu$ | = 2.32—4.12 | = 100 minutes at 23 | —21° C. |                             |
| 50—100      | = 4.12—5.53 | = 101               | „ „     | 21 —19.5                    |
| 100—200     | = 5.53—7.20 | = 97                | „ „     | 19.5—18                     |

And there is nothing comparable as regards time with such long periods anywhere in my broth cultures at constant temperatures, in the dark.

Unless, therefore, we assume that the air-temperatures given depart very widely, in different ways, from the real temperatures of the culture-drops, we must assume that the lower growth in the exposed culture was due to inhibition by the light rays.

Probably the most startling fact which comes out on examining these results is the extremely rapid growth of the dark culture from 6.41 onwards. To explain this it is necessary to point out one or two things not obvious at all. In the first place, as the filament grows longer there are more and more cells formed by the repeated bi-partition—*i.e.*, there are more and more elongated segments in the chain, and therefore a greater elongation of the whole as the sum of the growths of the individual cells becomes greater. Then, secondly, the vigour of the entire cell-series evidently increases as the feeding and other powers—*e.g.*, peptonising activity, &c.—increase.

Consequently, after a certain period these strongly developing filaments which have not been exposed to any disturbing agent, grow at

\* The doubling period is the time required for a given length of filament to double this length (see p. 403).

rates which are really enormous if we regard the relations of size. This is the more marvellous when one notices that this rapidity of growth was increasing *although the temperature was falling the whole time*.

This last is an important point, for if we now turn to the culture in the light, we find nothing like such a general increase in the power of development. Nevertheless, the filament clearly began to grow much more rapidly after sun-down (see 5.47 and 6.1 P.M.), and was evidently tending towards some such maximum at 7.23.

Now, why did not this filament begin to get up its maximum of growth as soon as the other? It was a *longer* and more vigorous specimen to begin with, for I purposely chose a germinal filament 25  $\mu$  long, whereas that in the dark was only 15  $\mu$  long—and unless some inhibiting agent was keeping it back, it seems incredible that its general curve of growth should ascend so slowly before sun-down.

But it seemed clear that the inhibition cannot be due to the temperature, because that was falling too slowly to produce such an effect; and, besides, it falls practically equally in both cases, so that if it checked the growth of this culture, one asks why the lowering of the temperature did not check it in the dark?

If, then, the growth is accelerated after sun-down, *in spite of a fall of temperature*—as we see to be the case—the logical conclusion seemed to be that the acceleration is due to the withdrawal of the inhibiting rays of light, and on the hypothesis that this is the case the curves are intelligible.

#### *Germination and Growth behind Glass Screens.*

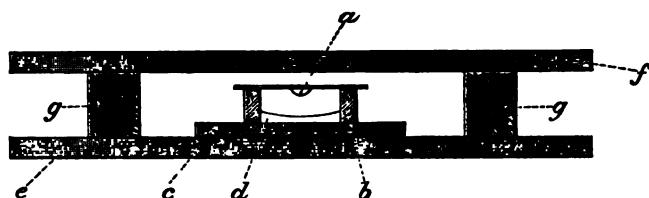
In view of the encouraging results of which the above is an illustration, it appeared worth while to further test the action of light on the germination and growth of this schizomycete, and this I did with the following measure of success.

The method employed was as follows:—A quantity of dilute broth in a test-tube was infected with the ripe spores to such an extent that the smallest drop I could manipulate with a platinum loop contained on the average at least about twelve and not more than about twenty-five spores.

I then made a number of hanging-drop cultures in the usual manner, on thin, sterilised cover-slips, each being at once luted to the moist chamber by means of sterile gelatine.

Each moist chamber was then placed on a piece of glass, plain or coloured, about 3½ inches square, and covered by a similar square of the same glass, supported just above the cover-slip from which the drop was suspended, by means of a suitable wooden block.

The following diagram gives the sectional view.



*a* = hanging drop, suspended from cover slip forming roof of moist chamber; *b* = glass cell in section; *c* = glass slide forming floor of moist chamber; *d* = layer of water to prevent hanging drop drying up; *e* = lower coloured or other glass screen; *f* = upper ditto, supported on wooden blocks shown in section at *gg*.

The glass screens were made sufficiently large to ensure that no light could possibly reach the moist-chamber that had not previously traversed them.

All being thus ready, the moist-chambers were placed in position, and the upper screen removed while I focussed the contents of the hanging-drop, and arranged so that the mirror of the microscope threw the light directly up, through the lower screen. The tube of the microscope was then drawn up so as to raise the objective sufficiently to allow of the upper screen being placed in position.

All the microscopes thus loaded were placed side by side at a south window, and illuminated exactly alike, and for equal periods; maximum and minimum thermometers being placed in position by them.

At stated intervals I examined all the cultures, and measured the growth, &c., of the filaments as follows:—In every case a number of filaments—usually five to ten—were measured, and an estimate formed of the commonest length that prevailed. Then I sought throughout the drop for the *longest* and for the *shortest* filament I could find.

In this way I was enabled to obtain a fair estimate of the amount of increment going on in each culture, as compared with a standard culture kept in the dark; for numerous trials had shown me that the spores germinate so freely, and the germinal rodlets in any one drop grow so evenly during the early periods, that the method may be trusted at least up to the point I push it to.

The following experimental series are typical, and serve very well to bring out the instructive results obtained by these comparative cultures.

To test the relative effects of strong blue and red light on the growth of the germinal filaments, two cultures were made in glass cells, as usual, and placed behind blue and red glass screens respectively.

The spores were sown at 10 A.M. and placed in the light over mirrors at a south window. The temperature was fairly steady at 16° C. The light was chiefly reflected from brightly illuminated clouds, but intermittent sunshine touched the mirrors from 11.35 A.M. to 1 P.M., at intervals.

At 4 P.M.—i.e., six hours after sowing, the spores behind the red screens had germinated to rodlets about 10  $\mu$  long, whereas no trace of germination could be observed behind the blue glass. At 8 P.M. the temperature having only fallen half a degree, the average length of the filaments behind the red glass was 125 to 135  $\mu$ , but none of the spores behind the blue glass had germinated out.

Next morning at 10 A.M. the filaments in the red light were 750 to 1,000  $\mu$  long, as near as I could measure them, but all the spores in the blue were dead, as evinced by their not germinating during the day at 14–18° C., nor during the two further days the culture was kept.

This experiment, and several others which confirmed it in every respect, shows clearly that in bright sun-light the spores are killed behind blue screens, whereas the same exposure behind red screens leads to no injury whatever—confirming the numerous similar results with macroscopic cultures.

On February 13 a bright, clear blue sky with strong sunshine prevailed all the morning, the afternoon being warm and with brilliant sunshine, obscured occasionally by clouds. I availed myself of the brilliant light reflected from the mirrors, but *only allowed direct sunshine* (reflected from the mirrors) *to play on the objects for a quarter of an hour* at the beginning of the exposure.

The spores had of course all been sown at the same time, so that each culture began under equal conditions. Six sowings were made, each containing about 20–30 spores in the hanging drop of dilute broth. One was kept under a darkened bell-jar, one behind ordinary glass, and the others behind red, blue, green, and pale olive glass respectively.\* The exposures began at 11 A.M. at 16° C.; the temperature rose somewhat rapidly to 18° C., and reached 20–21° C. before 2 P.M., and remained at that till after 4 P.M., when it slowly sunk to 16° C. at 8 P.M., and remained at that through the night.

At 2 P.M. all had begun to germinate, and by 5 P.M. perceptible growth of the filaments could be observed and measured, and so on later. I carried out the measurements as follows, each under the same scale, every division of which (with the power Zeiss D/4 used) corresponded to 5  $\mu$ .

In each drop I sought for the *longest* and the *shortest* filament or

\* I owe sincere thanks to Mr. Walker of the Normal School, South Kensington, and to Professor Rücker for much kindness in testing the diathermancy, transparency, and other properties of glass screens for me.

rodlet, a comparatively easy task with the small drops used and with this power, and noted the *commonest* average length which prevailed among the little plantlets developed. The following summary will give the best idea of the results. (Table, p. 315).

The averages were got by taking the sum of all the lengths measured, and dividing by the number of rods measured. This last number is placed in brackets next each average.

These results seemed to show very clearly that (1) the differences in the rapidity of growth are very large, and (2) they are not to be referred directly to the changes in temperature.

They also show that the growth in total darkness under the given conditions is much more rapid than the growth in any kind of light experimented with, except the red. It is *eventually* more rapid than in the red light, but not so *at first*, and I attributed the difference here to the fact that so long as the light was acting the dark heat rays in the red can make themselves felt to such an extent that the young filaments grow more actively during this period; when the light fails, however, between 6 and 7 P.M., the curve in the red falls—the effect of a falling thermometer, in fact.

In the present case the blue light seemed to be distinctly a retarding agent throughout, but it was not sufficiently *intense* to exercise its full effect, and the screen must be regarded as having acted more as a shade to the relatively low intensity of light employed than as a transmitter of any large proportion of the presumably injurious rays.

With the ordinary glass and the pale olive (which really transmits almost as much light) the retarding effects of the light seemed very evident. I attributed the more effective retardation behind the pale olive to the fact that it transmits about as strong a light as the ordinary glass, but with *relatively* more blue in its composition. The effects with green glass screens are often puzzling, as I have found in other cases. On the whole it seemed to act like a shade to the *intensity* of the light, but what does traverse has a lot of blue in it. It lets the dark heat rays through, and consequently the fall about 7 P.M. is like that of a fall of temperature.

In both cases—that of the red and that of the green—it will be noted that the fall of the curve would be aided by the fact that the temperature was also falling slowly all the time, and this latter fact also probably prevented the other curves (blue, ordinary, and pale olive) from ascending more rapidly from 7 P.M. onwards.

Without pressing this explanation too far, it seemed not unlikely that it was approximately correct, though the matter is undoubtedly a complex one.

In any case the above results seemed to show very clearly that whereas the mean growth of the filaments, in darkness, or behind

Relative Growths of Germinating Filaments in Darkness and in Various Coloured Lights.

| Treatment of culture. | Lengths at 2 P.M. in $\mu$ . |           |       | Lengths at 5 P.M. in $\mu$ . |           |       | Lengths at 7 P.M. in $\mu$ . |           |       | Lengths at 9.30 P.M. in $\mu$ . |           |       |
|-----------------------|------------------------------|-----------|-------|------------------------------|-----------|-------|------------------------------|-----------|-------|---------------------------------|-----------|-------|
|                       | Average.                     | Extremes. | Mean. | Average.                     | Extremes. | Mean. | Average.                     | Extremes. | Mean. | Average.                        | Extremes. | Mean. |
| Dark.....             | 6-10                         | 6-10      | 8.0   | 60.0 (5)*                    | 55-75     | 65.0  | Not taken†                   | 125-175   | 150.0 | 346.0 (4)                       | 200-450   | 325.0 |
| Ordinary glass        | 5.0 (6)                      | 4-5       | 4.5   | 28.0 (6)                     | 25-30     | 27.5  | 55.0 (11)                    | 20-85     | 52.5  | 102.0 (10)                      | 50-130    | 90.0  |
| Red.....              | 13.2 (5)                     | 12-15     | 13.5  | 108.75 (4)                   | 60-150    | 105.0 | 244.0 (11)                   | 100-350   | 225.0 | 270.7 (7)                       | 125-450   | 287.5 |
| Blue.....             | 5.0 (4)                      | 4-5       | 4.5   | 60.6 (4)                     | 45-80     | 62.5  | 144.5 (10)                   | 65-185    | 135.0 | 233.3 (9)                       | 75-340    | 207.5 |
| Green.....            | 9.0 (4)                      | 8-10      | 9.0   | 55.0 (6)                     | 35-80     | 57.5  | 101.87 (8)                   | 60-175    | 117.5 | 110.0 (7)                       | 60-200    | 180.0 |
| Pale olive....        | 3.0 (4)                      | 3-3.5     | 3.25  | 17.5 (4)                     | 15-25     | 20.0  | 36.6 (11)                    | 15-50     | 32.5  | 55.5 (10)                       | 30-70     | 50.0  |

\* The numbers in brackets in these columns give the number of measurements from which the average is taken.

† The average could not be taken here because a single filament was under observation at the time to obtain periodic records of its growth: hence the slide could not be searched all over.

red glass, which cuts out all the blue rays, at the temperature given, in  $10\frac{1}{2}$  hours, amounts to from 144 to more than 160 times the length of the spore, it is only 25 to 45 times its length in the same period if exposed for six or seven hours of the time to light passing through glasses (pale olive or ordinary) which transmit most or all of the blue rays. Under the same conditions spores behind glasses (blue and green) which act partly as shades which reduce the intensity of the light (and in so far favour growth), and partly as screens which so alter the composition of what light is transmitted that it consists of *relatively* large quantities of blue, show decided retardation of the growth—which in the given period amounted to from 65 to 104 times the length of the spore—act as might be expected, and the amount of growth is somewhere between the extremes.

In the following experimental series these facts seemed to come out still more prominently, principally perhaps because the temperature was somewhat higher and the insolation longer.

The arrangements were exactly as before, but the culture under pale olive glass had to be discarded, owing to partial drying up of the drop, because the gelatine luting was incomplete.

The spores were sown at 11 A.M., and the exposure began at 12 noon, the temperature being  $16^{\circ}$  C. The sun was bright and hot, and was directed on to each culture at the commencement for five minutes. An hour later, *i.e.*, at a period when the swelling spores would be just on the point of germinating, another five minutes' insolation was allowed to each, and so on through the afternoon, *i.e.*, five minutes at 2, 3, and 4 P.M. respectively.

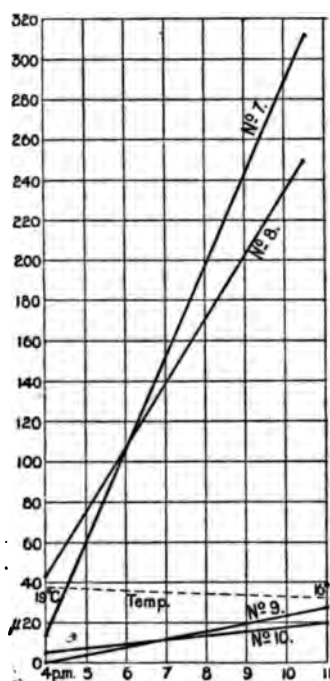
For the rest of the period the mirrors were carefully arranged, so that only bright light from the blue sky or from brilliantly illuminated clouds was allowed to fall on them.

The temperature steadily rose to  $18$ — $19^{\circ}$  C. about 1 P.M. to  $21$ — $22^{\circ}$  C. at 2 P.M., and to  $23$ — $24^{\circ}$  C. between 2 and 3 P.M. At 4 P.M. it had fallen to  $19^{\circ}$  C., and so down to  $16^{\circ}$  C. at 10 P.M., at which it remained through the night.

The first measurements were taken at 4 P.M., *i.e.*, after the spores had been sown five hours. As before, I examined each culture in the order of its making, so that although the hours given in the table are only the approximate mean times of the periods (about half an hour) occupied in examining the whole series, they give the correct interval in each case, whence no allowance need be made for the minute increments of elongation during those periods (see Curves 7—10).

## Relative Growths of Germinating Filaments in Darkness and in various Coloured Lights.

| Treatment of culture. | Length at 4 P.M. in $\mu$ .                  |           |       | Length at 10.30 P.M. in $\mu$ .   |           |       | Commonest length observed. |
|-----------------------|----------------------------------------------|-----------|-------|-----------------------------------|-----------|-------|----------------------------|
|                       | Average.                                     | Extremes. | Mean. | Average.                          | Extremes. | Mean. |                            |
| Dark ..               | 13.2(4)                                      | 10—16     | 14.0  | 296.85(8)                         | 225—400   | 312.5 | 275                        |
| Ordinary glass        | 4.5(4)                                       | 4—5       | 4.5   | 21.87(8)                          | 5—35      | 20.0  | 6                          |
|                       | But most have not yet germinated.            |           |       | Many spores still not germinated. |           |       |                            |
| Red ..                | 43.75(4)                                     | 30—55     | 42.5  | 286.25(8)                         | 100—400   | 250.0 | 300                        |
| Blue ..               | All spores swollen only, not yet germinated. | 15—25     | 20.0  | 28.86(11)                         | 7.5—50    | 28.75 | 30                         |
| Green ..              | 20.0(4)                                      | 15—25     | 20.0  | 334.3(8)                          | 300—450   | 375.0 | 250                        |



On comparing this table and curves with those last discussed, we find the following facts come out:—

The retarding action behind the blue and ordinary glass screens is much more marked, simply, no doubt, because the insolation was more intense. During the whole period (from 12 noon to 10.30 P.M.) of 10½ hours, although the time of exposure to light was shorter by

an hour at least, the filaments developed from the spores in darkness or behind the powerful shades (green and red glass) grew to from 125 to 187 times the length of the spore; those behind the blue and ordinary glass only grew to 10 to 15 times the length of the spore.

Moreover, it is interesting to observe that, so far as it goes (and I do not wish to press it unduly, because a larger number of measurements will be required to decide such a point) the red glass *again* acted as an incubator during the early stages, which I attribute to its acting as a trap for the dark heat-rays; and the more pronounced retarding action of the blue glass so long as the stronger light could penetrate it, fell somewhat after six or seven o'clock, when the darkness set in.

As before, I regarded the green as acting much like the red: it cuts off much of the *intensity* of the light, and in so far is a shade, while it transmits a light of such composition that the dark heat rays are relatively in excess, and still some blue passes. Nevertheless, there is something puzzling about the behaviour of this green glass which I cannot as yet explain. The same uncertainty of its action prevails in other cases, as I have already stated.

To test still further the action of various coloured screens, I repeated the experiments with glass screens, taking a series of comparative thermometrical readings at the same time. The days chosen (February 18—19) were very bright and clear, and great care was taken to obtain the light used from the clear blue sky, to the east of the sun, so as not to run any risk of direct insolation at unexpected periods.

Moreover, before beginning the experimental exposures, I made a series of observations by placing the bulbs of thermometers between the glass screens exactly in the place occupied by the moist culture-chambers, and exposed them under all conditions.

When the thermometer lying on the table under the south window used for exposures, registered  $25^{\circ}\text{C}$ . in the sun, I found that the one with its bulb over the mirror of the microscope registered  $27^{\circ}\text{C}$ ., when the light reflected from the blue sky at noon passed through the blue glass: on then reflecting the sun's image on to the same bulb between the blue glasses, the temperature, of course, rapidly rose, and was at  $32^{\circ}\text{C}$ . ten minutes later.

On repeating this with the bulb of a thermometer between the red glasses, the temperature rose to  $33^{\circ}\text{C}$ . in the same time (10 minutes), and rapidly fell to  $28^{\circ}\text{C}$ . when the mirror reflected only light from the blue sky to the east of the sun.

With the orange glasses, the temperature shot up to  $33^{\circ}\text{C}$ . in the sun's image, and fell to  $27^{\circ}\text{C}$ . in 10 minutes on turning the mirror as before; and with the green glasses, the temperature rose to  $30^{\circ}\text{C}$ . in the sun, and fell to  $27^{\circ}\text{C}$ . in the rays from the blue sky.

It was evident that, provided I kept to the reflected rays from the blue sky near the sun, the heating effect was about 2—3° higher than in the open air beside the microscope, and that the cultures between red glasses were exposed to at least about 1° or 2° higher than those between the blue, orange, or green glasses, and these facts must be carefully borne in mind when discussing the results.

The hanging drops, each containing about 25 spores, were made at 11.30 A.M., from broth, infected at 9 A.M., and kept at 15° C., so that the spores were already swelling and ready to begin germinating when the cultures were exposed at 12.30 noon.

As soon as the cultures were placed in position on the microscopes, I turned each mirror so as to powerfully insolate each culture for exactly five minutes; that was all the direct sunlight they were allowed during the whole period, except such rays as struck the top glass obliquely, and the heating effect of which must be taken into account in what follows.

The results of the four examinations I made of these cultures at about 2.45, 4.45, 7.0, and 10.46 P.M. respectively, showed considerably more growth in the red and orange than in the blue and green.

The culture in diffuse light was an accident, and not part of the planned series. It stood in a shaded part of the room, and was not only at a higher temperature during the hours of exposure, but was considerably more advanced to start with. It cannot, therefore, be compared with the others, and must be regarded as merely of passing interest: its curve fell rapidly after 7 P.M., possibly owing to the fall of temperature.

Two hanging drop cultures of broth with a trace of gelatine, of equal age—spores sown at 10—12° C. over night—were placed on microscopes in the south window arranged as follows:—The cells had a thin glass floor.

One had a sheet of red glass below, and a similar sheet above; the other a sheet of blue glass below, and screens of the same in front, so that all light direct from the sky had to traverse the glasses before reaching the drops. The only difference between the two arrangements (apart from the colour of the glasses) was that the first one had its second sheet of red glass close over the drop. This difference was necessary owing to the microscopes being of different patterns.

Unfortunately the morning turned out cloudy, with rain at intervals, and occasional short breaks of sunshine; a high wind kept the temperature down to about 17—18° C., rising now and then to 19—20° C.

It will also be noted that I here exposed, not *spores*, but rodlets already strongly germinated out. When the red culture was started, at 10.45 A.M., the rodlet selected (to be kept under observa-


tion the whole time) was  $30\ \mu$  long; its growth in length was recorded every 15 to 30 minutes or so, and the temperature as shown by a thermometer by the side of the cultures noted.

After allowing the growth to go on for  $1\frac{1}{4}$  hours in the red, I changed the glasses, and put the blue ones here and the red ones on the other culture; after growing thus for  $2\frac{1}{4}$  hours, the exchange to the original glasses was made again.

The blue culture was started three-quarters of an hour later than the red, a rodlet of almost the same length ( $31.5\ \mu$ ) being selected for observation.

After being half an hour under the blue glasses, this was changed to red, and allowed to grow in red light for  $2\frac{1}{4}$  hours.

The result was not sufficiently decisive to encourage my drawing any conclusion. It is true the rod growing under red glass seemed to have got a good start, so that its ultimate sojourn in blue light was less injurious than might, perhaps, have been expected; while the shorter stay of the other in blue light appeared nevertheless long enough to have affected it seriously. But meanwhile other factors complicated the matter, and I discarded this line of experiment. Before passing to the further developments, however, it will be well to see what the foregoing results amount to.

1. It is clear that growth occurs with increasing rapidity in suitable food materials and under suitable conditions, and is evidently intercalary. Since it comes to an end eventually, the curve must be that of a long drawn out .

2. The measurements show, also, that the rate of growth may vary owing to the action of certain factors, partly external and partly internal; and these variations may be local in a filament, or general.

3. The experiments show that the growth is slower on exposure to sunlight (and may even be stopped altogether) passing through certain screens, whereas it is not retarded perceptibly in light through other screens.

4. The results obtained so far, however, do not decide with certainty how far the differences with *growing filaments* depend on other factors than the light; though they are pretty definite as regards the *spores*, exposed in a resting condition. They even suggest that there may be differences in the two cases—*e.g.*, that the actively growing filaments may have some power of overcoming the detrimental action of rays which seriously injure, or even kill, the spores in a state of rest.

5. It may be added that there is no question that exposure to *direct* sunlight kills both spores and filaments; numerous trials have proved that. The question under investigation is rather how far lower intensities of light are efficacious in this respect. So far the measurements did not satisfactorily prove conclusively that active

growth once in full swing is retarded, during the short period I am able to measure it, by a low intensity of light independent of temperature.

*Criticism of the foregoing Results, and Experiments on Temperatures.*

The more I became familiarised with the methods which had given the foregoing results, the more evident it became that, instructive and valuable as the facts may be—and there can be no question as to their decisive confirmation of the conclusions of previous observers, as well as myself, that direct sunshine kills so long as the blue rays are not filtered out—they are not altogether satisfactory. The variations due to internal causes are so minute that, real though they are, they do not seriously affect the measurements of growth, provided I compare similar rods or long filaments during the same periods, and it is obvious that if I use cultures in the same food-material, and prepared alike, there can be no serious drawback in the method so far as *comparative* results go.

But over and over again I was impressed by the dangers possible as regards the screens employed. It is obvious that with an organism like this, very sensitive\* to changes of temperature during its growth, differences of a few degrees at the critical periods might easily lead one into errors which, if neglected, would vitiate many of the conclusions entirely.

The matter is a serious one, for it must be remembered that biologists, and especially botanists, have long been in the habit of using coloured screens, and if these agents are responsible for changes of temperature not recorded in the observations, we are in danger of making fundamental errors.

The suspicion that the temperatures, as recorded merely by thermometers in the air beside the microscopes, might be less trustworthy guides to the changes of temperature going on in the hanging drop-cultures in the glass cells than had been hitherto assumed, now forced itself upon me; and with that, of course, the idea that I might be running risks of confusing the effects of high or low temperatures with those of light, became painfully disturbing.

In any case, it was obviously necessary to institute a series of experiments to test these notions, and it was well I did so, for the results, although not wholly unexpected, were more startling than I care to dwell on now.†

\* It was not till a much later period that I found how extremely sensitive it is to temperature changes, as will be seen subsequently.

† It scarcely needs mention that the question here concerned is not the physical one whether two different screens transmit heat-rays in different proportions, but the physiological one how far the differences affect the organism experimented on. It can obviously be decided only by patient comparative investigations.

I select the following series of observations in illustration from a large number which I made for my own guidance: it does not seem necessary to publish all the notes, but as many readers may be unacquainted with the magnitude of the dangers referred to, I retain one or two experiments of a somewhat obviously elementary nature.

In order to understand the behaviour of the thermometer as an indication of what was going on in the culture-chambers, it was clearly necessary that I should make a series of blind experiments in which the bulbs of the thermometers were as carefully placed in the relative proper positions as possible.

The following series refer to the sort of temperature changes to be expected in the laboratory, near a south window, in spring.

*Series 1 (an extreme Case).*

Changes of temperature as indicated by thermometers (A) lying on table beside microscopes; (B) hanging in glass bell-jar exposed to light; (C) hanging in darkened bell-jar; (D) on a stool in the shade and much nearer the hot-water pipes which warmed the room.

| Time.      | Therm. on table. | Therm. in lighted bell.               | Therm. in dark bell. | Therm. on stool in shade. |
|------------|------------------|---------------------------------------|----------------------|---------------------------|
|            | ° C.             | ° C.                                  | ° C.                 | ° C.                      |
| 8.30 A.M.  | 11.0             | 14                                    | 14.0                 | 13.5                      |
| 10.0 "     | 15.0             | 17                                    | 16.0                 | 16.5                      |
| 10.30 "    | 16.25            | 22 (sun on bulb; fell to 19 when off) | 17.5                 | 17.0                      |
| 11.0 "     | 17.25            | 19                                    | 18.0                 | 17.25                     |
| 11.30 "    | 17.25            | 22                                    | 20.0                 | 17.25                     |
| 12 noon    | 22.5 } sun       | 25 } sun on                           | 25.0                 | 19.75                     |
| 12.40 P.M. | 30.0 } on        |                                       |                      |                           |
| 1.0 "      | 30.0 } bulb      | —                                     | —                    | —                         |

Obviously this kind of thing would not do for the experiments contemplated, and the first thing I set myself to overcome was the variations due to direct insolation, for it was found that the temperature of the air in the room used is remarkably constant, from the regular action of the heating apparatus, and the rise in temperature during the day and the fall during the night are comparatively small and slow, provided the nights are not frosty and that the venetian blinds are drawn in the evening.

*Series 2 (Average Cases).*

Thermometer readings when the bulb is placed between blue and red glasses, over the mirror of the microscope and in the position of the hanging drop.

(A.) Bulb not Blackened.

| Time.      | Blue. | Red. | Remarks.                                                 |
|------------|-------|------|----------------------------------------------------------|
|            | ° C.  | ° C. |                                                          |
| 11.15 A.M. | 17·5  | 17·5 | Sun not yet on window.                                   |
| 12.25 noon | 23·5  | 23·0 | Sun on glasses.                                          |
| 12.40 P.M. | 21·0  | 21·0 | Top glass shaded by cardboard.                           |
| 1.0 "      | 25·0  | 23·0 | Top shaded, but mirror in full sun. Hot glare, but hazy. |
| 1.10 "     | 27·5  | 28·0 | Full exposure to sun above and below.                    |
| 2.0 "      | 21·0  | 21·0 | Sun off; full exposure to light from sky only.           |
| 2.20 "     | 20·5  | 20·0 |                                                          |
| 2.40 "     | 20·0  | 19·5 |                                                          |
| 3.15 "     | 19·5  | 19·0 |                                                          |
| 3.45 "     | 18·75 | 18·5 |                                                          |
| 4.5 "      | 18·5  | 18·0 |                                                          |
| 4.40 "     | 18·25 | 18·0 |                                                          |
| 5.30 "     | 17·75 | 16·5 |                                                          |

(B.) Bulb Blackened.

| Time.      | Blue. | Red.  | Remarks.                                                               |
|------------|-------|-------|------------------------------------------------------------------------|
|            | ° C.  | ° C.  |                                                                        |
| 10.0 A.M.  | 15·0  | 15·0  | Temperature of air at time of beginning.                               |
| 10.45 "    | 17·0  | 16·75 | Sun not yet on window. Plane mirrors and ordinary light.               |
| 11.0 "     | 17·0  | 16·75 |                                                                        |
| 11.45 "    | 18·75 | 18·5  |                                                                        |
| 11.55 "    | 25·0  | 23·5  | Result of turning plane mirror to full sun (which also glares on top). |
| 12.45 P.M. | 24·5  | 24·0  | Sun full on (very hazy sun all day).                                   |
| 1.0 "      | 22·75 | 22·5  | Light with plane mirror from sky east of sun.                          |
| 1.45 "     | 22·0  | 21·5  | Sun sinking behind roof of building to south.                          |
| 1.50 "     | 21·0  | 20·25 |                                                                        |
| 1.56 "     | 20·0  | 19·5  |                                                                        |
| 2.35 "     | 18·5  | 17·5  |                                                                        |
| 3.0 "      | 18·0  | 17·25 |                                                                        |
| 3.40 "     | 18·0  | 17·5  |                                                                        |
| 3.50 "     | 18·0  | 17·5  |                                                                        |
| 4.10 "     | 17·75 | 17·0  |                                                                        |

These examples will suffice to show how far the screens differ as regards transmission of heat rays.

The following series is interesting as showing that the thermometer hanging in the glass bell-jar, with its bulb close to the culture-chamber, may undergo considerable variations without any corresponding reaction in the growth curvatures.

The experiment was designed to see if the light of a paraffin lamp would be sufficiently active to affect the growth; but the results showed that—for short periods, at any rate—this is not the case. As is well known, however, the flame of a paraffin lamp is very hot, and the possibility arises that the rise of temperature may mask the effect, if the paraffin light has any.

I give the results in any case, though they prove little by themselves: they only show that, either (1) the organism does not *rapidly\** react to any great extent to quick, small, but considerable, variations of the temperature immediately around the culture-chamber, or (2) the inhibiting action of the light is sufficient to just undo or mask that effect.

The plan was as follows. A culture drop was prepared as usual, and germination allowed to proceed in the dark for several hours.

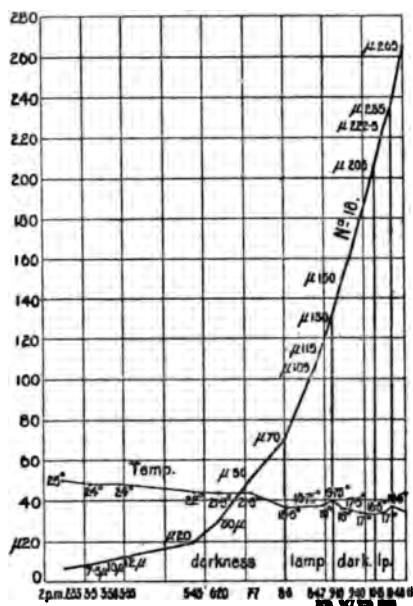
At 12 noon, a germinal filament 5  $\mu$  long was selected for observation. The temperature of the interior of the darkened bell-jar, as shown by a thermometer hanging with its bulb close to the culture, was 24° C. The reading and measurement being taken, I left the whole system undisturbed till after 2 o'clock, except to take the temperature readings given in the following tabular *résumé*:—

| Time.           | Temp.  |
|-----------------|--------|
| 12 noon .....   | 24° C. |
| 12.18 P.M. .... | 25     |
| 12.40 „ .....   | 28     |
| 1.0 „ .....     | 28     |
| 1.45 „ .....    | 27.5   |

At 2.33 the sun had passed off the window (and it was the direct insolation playing on the wrappings which so raised the temperature of the interior of the bell-jar), and the temperature at once fell to 25° C., and measurements were at once begun, as shown in the following table (see Curve 18).

|          | Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|----------|-----------|---------|-----------|---------|---------|-------|
|          |           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
|          | 2.33 P.M. | 6       | —         | —       | —       | 25    |
|          | 3.5 „     | 7.5     | 32        | 1.5     | 0.05    | 24    |
|          | 3.30 „    | 10.0    | 25        | 2.5     | 0.1     | 24    |
|          | 3.55 „    | 12.0    | 25        | 2.0     | 0.08    | 24    |
|          | 5.45 „    | 20.0    | 110       | 8.0     | 0.07    | 22    |
|          | 6.20 „    | 30.0    | 35        | 10.0    | 0.3     | 21.5  |
|          | 7.7 „     | 50.0    | 47        | 20.0    | 0.42    | 21.5  |
|          | 8.0 „     | 70.0    | 53        | 20.0    | 0.38    | 18.5  |
| In light | 8.47 „    | 105.0   | 47        | 35.0    | 0.74    | 18.75 |
| of lamp  | 8.56 „    | 115.0   | 9         | 10.0    | 1.1     | 19    |
|          | 9.10 „    | 130.0   | 14        | 15.0    | 1.0     | 19.75 |
|          | 9.27 „    | 150.0   | 17        | 20.0    | 1.18    | 18    |
|          | 9.40 „    | 160.0   | 13        | 10.0    | 0.77    | 17.5  |
|          | 9.56 „    | 180.0   | 16        | 20.0    | 1.25    | 17    |
| In light | 10.15 „   | 205.0   | 19        | 25.0    | 1.66    | 16.5  |
| of lamp  | 10.30 „   | 222.5   | 15        | 17.5    | 1.16    | 17.0  |
|          | 10.40 „   | 235.0   | 10        | 12.5    | 1.25    | 18.5  |
|          | 11.0 „    | 265.0   | 20        | 30.0    | 1.5     | 17    |

\* It does react, however, as the curves show; but the result is not very great in short periods.



It will be noted that the evident rise of temperature from 8:47° 9:10° was responded to by increased growth.

The following gives an idea of the differences in the curves observed on a hazy day, when the sun was very hot, but could be look

#### Light.

| Time.      | Length. | Interval. | Growth. | Rate. | Temp. |
|------------|---------|-----------|---------|-------|-------|
|            | μ.      | mins.     | μ.      | μ.    | ° C.  |
| 9.53 A.M.  | 8.0     | —         | —       | —     | 17.0  |
| 10.12 "    | —       | —         | —       | —     | 17.5  |
| 10.15 "    | —       | —         | —       | —     | 18.0  |
| 10.25 "    | —       | —         | —       | —     | 18.0  |
| 10.55 "    | 10.0    | 62        | 2.0     | 0.03  | 18.0  |
| 11.17 "    | —       | —         | —       | —     | 18.0  |
| 12.28 P.M. | 12.0    | 95        | 2.0     | 0.02  | 24.0  |
| 12.42 "    | 26.0    | 14        | 14.0    | 1.0   | 23.0  |
| 1.6 "      | 35.0    | 24        | 9.0     | 0.37  | 23.0  |
| 1.58 "     | 80.0    | 52        | 45.0    | 0.96  | 22.5  |
| 2.20 "     | 105.0   | 22        | 25.0    | 1.1   | 21.75 |
| 2.40 "     | 125.0   | 20        | 20.0    | 1.0   | 21.0  |
| 3.9 "      | 155.0   | 29        | 30.0    | 1.0   | 20.5  |
| 3.40 "     | 167.5   | 81        | 12.5    | 0.4   | 20.0  |
| 4.1 "      | 185.0   | 21        | 17.5    | 0.8   | 19.5  |
| 4.36 "     | 205.0   | 35        | 20.0    | 0.6   | 19.5  |
| 5.30 "     | 230.0   | 54        | 25.0    | 0.46  | 18.0  |
| 6.15 "     | 250.0   | 45        | 20.0    | 0.44  | 18.0  |

## Dark.

| Time.      | Length. | Interval. | Growth. | Rate.   | Temperature.  |
|------------|---------|-----------|---------|---------|---------------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 9.50 A.M.  | 8.0     | —         | —       | —       | 17.0          |
| 10.5 "     | 9.0     | 15        | 1.0     | 0.07    | 17.0          |
| 10.25 "    | —       | —         | —       | —       | 17.5          |
| 10.54 "    | 10.0    | 49        | 1.0     | 0.02    | 18.0          |
| 11.17 "    | —       | —         | —       | —       | 18.0          |
| 12.29 P.M. | 12.0    | 35        | 2.0     | 0.06    | 20.0          |
| 12.50 "    | 20.0    | 21        | 8.0     | 0.38    | 20.75         |
| 1.7 "      | 27.5    | 17        | 7.5     | 0.44    | 21.0          |
| 2.0 "      | 45.0    | 53        | 17.5    | 0.33    | 21.0          |
| 2.21 "     | 55.0    | 21        | 10.0    | 0.5     | 20.75         |
| 2.42 "     | 70.0    | 21        | 15.0    | 0.7     | 20.5          |

at without protecting the eyes. One culture was under an ordinary bell-jar exposed to light, the other under a similar bell darkened with black paper, the thermometer bulbs being placed in the cavities of dummy cells, arranged as the culture cells in each case.

It would have been possible here to suppose that the light actually favoured the growth if other facts were not known.

By an accident, this filament was lost, and another (in the same cell) had to be selected for the continuation of the observations, which were resumed without any serious interference with the conditions at 3.13. The following are the results:—

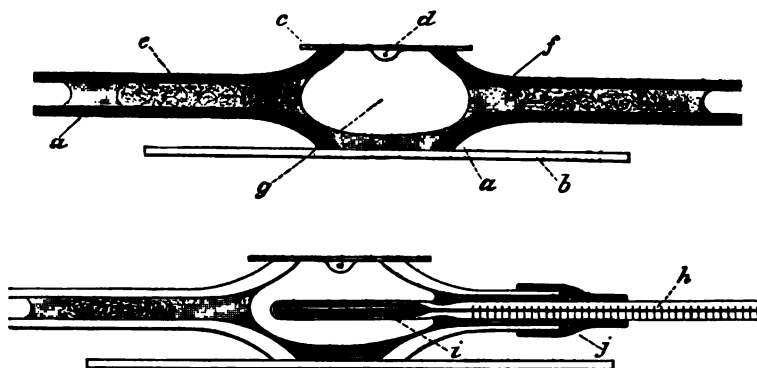
## Dark.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temperature.  |
|-----------|---------|-----------|---------|---------|---------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 3.13 P.M. | 55      | —         | —       | —       | 20.0          |
| 3.43 "    | 75      | 33        | 20      | 0.6     | 20.0          |
| 4.3 "     | 90      | 20        | 15      | 0.75    | 19.5          |
| 4.37 "    | 105     | 34        | 15      | 0.44    | 19.0          |
| 5.32 "    | 160     | 55        | 55      | 1.0     | 18.0          |

Here it is probable the rise of temperature in the lighted cell was responsible for the enhanced growth; and it may be concluded that if the light of this hazy day had any inhibitory effect, it was more than covered by the stimulus given to the organism by the heat-rays. It is by no means to be concluded with certainty that no inhibition occurred—only that none could be observed during the comparatively short period in which measurements are possible owing to the complication referred to.

The following set of experimental observations were made to test more accurately the heating of the cells, and the rise of temperature in the cultures, as compared with the temperature of the thermometer merely between the glasses, and that in the air close to the culture.

One of my moist chambers was prepared exactly as for a culture except that one of the two arms was cut short so that the bulb of thermometer could be inserted into the cavity as shown in the accompanying figure.



*a* = cavity of the glass cell (moist chamber) the atmosphere in which is kept saturated by water-vapour which evaporates from the layer of water *b* and with cotton-wool plugs *c* and *c'*. *d* the hanging-drop suspended from the cover-slip. The latter is luted to the glass-cell by stiff gelatine; while the floor of the cell is formed by a glass slide (*e*) cemented by paraffin melting between 55° and 60° C.

At *c'* the second open arm of the glass cell has been cut short to receive the bulb of the thermometer *f* (the bulb of which may be blackened). *g* and *g'* are two coloured glasses, so placed that all the light reaching the hanging-drop *d* must pass through them.

Having arranged such a cell over the mirror of the microscope, and loaded another microscope with an exactly similar thermometer whose bulb (blackened or not as necessary) is merely suspended between glasses similar to *g* and *g'*, I proceeded to compare the temperatures on a hot bright morning, at a south window, with those registered by thermometers hanging from the microscopes, and lying on the table at the base.

In the following table, the records of the thermometers outside the microscopes are given in the first column; those whose bulb was merely supported between the coloured glasses in the second (under the heading "dummy"); and that whose bulb was in the culture cell under the third column. The bulbs in second and third columns were blackened, the others not.

The coloured glasses used were the ruby ones marked in table. The thermometers in the air were exposed to the sun, and so the temperatures given are not really those of the air, but they serve my purpose for this preliminary experiment, which is only intended to give an instance of the enormous variations which may be got if the observer is not on his guard against insolation.

| Time.      | Air.      | Dummy. | Cell. | Remarks.                                                                                  |
|------------|-----------|--------|-------|-------------------------------------------------------------------------------------------|
|            | ° C.      | ° C.   | ° C.  |                                                                                           |
| 10.50 A.M. | 17.5      | 17.0   | 16.5  | Concave mirror, and light from blue sky to west of rising sun.                            |
| 11.0 "     | 18.0      | 17.25  | 16.75 |                                                                                           |
| 11.1 "     | 12.0—13.0 | 16.5   | 16.0  |                                                                                           |
| 11.2 "     | 10.5—11.0 | 16.5   | 14.5  | Opened window and let in cold wind.                                                       |
| 11.15 "    | 13.5—14.5 | 19.0   | 18.0  | Open window. Full glare of sun on coloured glasses and mirrors, through the window glass. |
| 11.18 "    | 15—16     | 22.0   | 19.0  |                                                                                           |
|            |           |        |       |                                                                                           |
| 11.24 "    | 16.5      | 31.0   | 30.0  | Same arrangement, but sun's glare with no glass of window intervening.                    |
| 11.26 "    | 16.5—17.5 | 35.5   | 38.0  |                                                                                           |
|            |           |        |       |                                                                                           |
| 11.30 "    | 18.5      | 45.0   | 44.0  | Most intense direct glare from sun I could get, with no glass intervening.                |
| 11.32 "    | 18.5—19.0 | 45.5   | 45.0  |                                                                                           |
| 11.33 "    | 19.0      | 47.0   | 46.0  |                                                                                           |
| 11.34 "    | 19.5—20.0 | 47.5   | 45.0  | Arrangement same, but shaded sun from top coloured glass.                                 |
| 11.37 "    | 20.0      | 48.0   | 45.0  |                                                                                           |
| 11.38 "    | 20.0      | 48.0   | 44.0  |                                                                                           |
| 11.40 "    | 20.0      | 40.5   | 35.0  | Upper red glass exposed to direct sun. Mirrors taking light from blue sky to east of sun. |
| 11.41 "    | 19.0      | 35.5   | 30.5  |                                                                                           |
| 11.42 "    | 18.0      | 32.5   | 29.0  |                                                                                           |
| 11.44 "    | 19.0—19.5 | 30.5   | 27.5  |                                                                                           |
| 11.45 "    | 19.5      | 30.0   | 27.5  |                                                                                           |

On looking at these results, one or two interesting facts of considerable importance to the investigation come out and serve as a basis for great caution in exposures of this kind.

In the first place it is clear that direct insolation causes such enormous rises of temperature in the cells, that no results could be expected sufficiently definite to be of use unless these could be controlled. Secondly, it is equally clear that the variations of temperature in such a south window are too sudden and great to be neglected as serious sources of error; and, thirdly, the ordinary mode of recording the temperature by means of thermometers merely hanging near the cultures obviously gives very imperfect results, and cannot be trusted for any such purposes as these.

On the other hand, the records as given by the blackened-bulb thermometer in the dummy and model cells (especially the latter) may be taken as true indications of the temperature in the culture-drops, and it is instructive to observe that the culture-cells respond *slowly* to alterations in the temperature of the air, but *very rapidly* to

changes in the intensity of the sun-light when direct on the mirror.

That the sluggishness in responding to the air-temperature is due to the protection of the glass and the layer of water will be accepted at once, I take it, and it will be equally obvious that the sharp rise in direct sunlight is owing to the dark heat rays, reflected from the mirror, passing easily through the red glass and water layer and being absorbed direct.

On the whole, then, we may conclude that so long as the culture-cells are protected from the *direct* rays of the sun, the temperature of their interior does not quickly vary, though it may be somewhat higher than that of a thermometer outside.

By keeping a thermometer, with blackened bulb, in such a culture-chamber side by side with the growth culture under examination, however, it may be accepted that the temperature of the growing schizomycete in the hanging drop is very accurately known.

Three culture chambers, with hanging drops of broth, were prepared over-night, the only difference introduced being that I employed thin *quartz* instead of glass for the floors. The spores germinated out slowly, at 10—12° C. during the night, and the filaments began growing more and more quickly as the temperature rose in the morning.

By 10 A.M. I had prepared a control or "dummy" cell, with the blackened bulb of a thermometer in it, as described on p. 327, and placed this beside two of the cultures, all in position on their microscopes, at the south window. One culture was arranged for exposure to light, and a thermometer (blackened bulb) hung to its microscope. The other culture was arranged in exactly the same way, but kept darkened by a dome of thick brown paper.

The third culture was placed in a cooler room at a north window, for growth at a lower temperature. At both windows ordinary thermometers were also placed to give the air temperatures in the usual way.

Unfortunately the day turned out dull and cloudy, with rain occasionally, but now and then the sun shone brightly at intervals during the morning.

The following tables give the results. Small thick cardboard slips were arranged as shutters to prevent the direct sun-light from falling on the mirrors or thermometers, &c., but with as little shutting off of light as possible.

## Dark.

| Time.      | Length. | Interval. | Growth. | Rate.   | Temperature of air.  |
|------------|---------|-----------|---------|---------|----------------------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . |
| 10.46 A.M. | 43.5    | —         | —       | —       | 17.5                 |
| 11.1 "     | 47.85   | 15        | 4.35    | 0.29    | 17.5                 |
| 11.16 "    | 53.50   | 15        | 5.65    | 0.38    | 17.5                 |
| 11.31 "    | 58.72   | 15        | 10.87   | 0.72    | 17.5                 |
| 11.46 "    | 67.42   | 15        | 8.70    | 0.58    | 17.0                 |
| 12.12 P.M. | 82.65   | 26        | 15.23   | 0.58    | 17.5                 |
| 12.31 "    | 95.70   | 19        | 13.05   | 0.70    | 17.5                 |
| 12.54 "    | 115.27  | 23        | 19.57   | 0.85    | 16.5                 |
| 2.2 "      | 201.40  | 68        | 86.13   | 1.26    | 21.0*                |
| 2.26 "     | 238.25  | 24        | 36.85   | 1.53    | 19.0                 |

\* At 2.2 P.M. I turned the light from the cloudy sky on, taking care to shade from any danger of direct sun, of which, indeed, there was none now.

Total growth = 194  $\mu$  in three hours forty minutes, which at constant rate = 0.88  $\mu$  per minute.

Growth to 12.54 P.M. = 71.77  $\mu$  in 128 minutes, which at constant rate = 0.56  $\mu$  per minute.

Growth afterwards = 123  $\mu$  in ninety-two minutes, which gives an average of 1.3  $\mu$  per minute.

## Light.

| Time.      | Length. | Interval. | Growth. | Rate.   | Temperature of air.  | Temperature "in dummy" cell. |
|------------|---------|-----------|---------|---------|----------------------|------------------------------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . | $^{\circ}\text{C}$ .         |
| 10.45 A.M. | 73.95   | —         | —       | —       | 17.0                 | 17.0                         |
| 11.0 "     | 80.91   | 15        | 9.96    | 0.66    | 17.5                 | 17.5                         |
| 11.15 "    | 89.17   | 15        | 8.26    | 0.55    | 17.0                 | 17.5                         |
| 11.30 "    | 97.87   | 15        | 8.70    | 0.58    | 16.0                 | 17.0                         |
| 11.45 "    | 108.75  | 15        | 10.85   | 0.72    | 16.5                 | 17.5                         |
| 12.6 P.M.  | 121.80  | 21        | 13.05   | 0.62    | 17.0                 | 18.5                         |
| 12.30 "    | 138.20  | 24        | 16.40   | 0.68    | *20.0                | 18.5                         |
| 12.52 "    | 154.42  | 22        | 16.22   | 0.74    | 15.0                 | 17.0                         |
| 2.0 "      | 217.50  | 68        | 63.08   | 0.92    | 20.0                 | 20.0                         |
| 2.30 "     | 281.0   | 30        | 43.50   | 1.45    | 20.0                 | 21.0                         |

\* The sun was accidentally allowed to catch the bulb: on shading it, it fell to 17 $^{\circ}\text{C}$ . in one minute.

Total growth = 187  $\mu$  in three hours forty-five minutes, which at constant rate = 0.83  $\mu$  per minute.

Growth to 12.52 P.M. = 80.5  $\mu$  in 127 minutes, giving an average of 0.63  $\mu$  per minute.

Growth afterwards = 106.5  $\mu$  in ninety-eight minutes, giving an average of 1.08  $\mu$  per minute.

## Third Culture, at Lower Temperature at North Window.

| Time.      | Length. | Interval. | Growth. | Rate.   | Temperature of air (bulb not blackened). |
|------------|---------|-----------|---------|---------|------------------------------------------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C.                            |
| 9.50 A.M.  | 33      | —         | —       | —       | 12.5                                     |
| 10.25 "    | 42      | 35        | 9       | 0.26    | 12.5                                     |
| 10.40 "    | 45      | 15        | 3       | 0.20    | 13.0                                     |
| 10.53 "    | 49      | 13        | 4       | 0.30    | 14.0                                     |
| 11.18 "    | 54      | 25        | 5       | 0.20    | 14.5                                     |
| 11.32 "    | 60      | 14        | 6       | 0.40    | 14.5                                     |
| 11.48 "    | 64      | 16        | 4       | 0.25    | 14.5                                     |
| 12.14 P.M. | 72      | 26        | 8       | 0.30    | 14.5                                     |
| 12.36 "    | 81      | 22        | 9       | 0.40    | 14.5                                     |
| 12.58 "    | 88      | 22        | 7       | 0.30    | 14.5                                     |

Total growth = 55  $\mu$  in three hours eight minutes, which at constant rate = 0.29  $\mu$  per minute.

The scrutiny of these tables can leave little doubt that the general shape of the curves is due to the temperature, and the curve at the lower temperature is hardly useful for any other purposes.

As regards the other two curves, however, the sharper ascent of the curve of the dark culture from 1 o'clock to 2.30 requires explanation, and, unfortunately, the data do not quite suffice for it. Of course, the suspicion arises that it may be due to the inhibiting action of light so retarding the growth in the one case, that the darkened filament—although a younger, and therefore more feebly growing specimen to begin with—is gradually making way and more than catching it up.

The following comparative observations were made with the dummy cells, between  $\text{CuSO}_4$  and bichromate screens, top and bottom, on a bright day in March.

Here, again, it is evident that the observer must be very carefully on his guard when using these screens, of glass bottles containing liquids which filter out different rays, of different heating power; and I would also call attention to the matter of the heating up of these screens in the sun and their power of slowly radiating their heat later on.

| Time.           | Temperature of cell in |                 |
|-----------------|------------------------|-----------------|
|                 | Blue.                  | Orange.         |
| 10.0 A.M. ....  | 14.5 $^{\circ}$        | 15.0 $^{\circ}$ |
| 11.30 " .....   | 21.0                   | 22.0            |
| 12.15 P.M. .... | 19.0                   | 20.0            |
| 12.30 " .....   | 21.0                   | 22.0            |

(On exposing the dummy cells to the full glare of the sun the temperatures ran up to 25° (blue) and 32° (orange), and on shading they fell more slowly to 22° (blue) and 23° (orange).)

|             |       |       |
|-------------|-------|-------|
| 1 P.M. .... | 19·0° | 20·0° |
| 2 „ ....    | 22·0  | 23·0  |

(On full exposure the blue at once ran up to 25° C., and returned on shading to 23·5°; the orange ran up to 33° C., and returned to 24·5°.)

|                      |       |       |
|----------------------|-------|-------|
| 5 P.M. ....          | 17·0° | 16·0° |
| 10 „ ....            | 12·5  | 13·5  |
| 9 A.M. next morning. | 13·0  | 13·5  |

The following series were made with the screens below the cells, the thin glass roofs would therefore be exposed to radiation, &c., thus accounting for the more rapid changes—in part at any rate. This will probably suffice for calling attention to this point, which must also be carefully borne in mind. I shall have opportunities of referring to these and other matters of the kind subsequently; but for the present this and the foregoing illustrations of the dangers may suffice.

| Time.      | Remarks.                                | Temperature of dummy cell behind screen. |             |
|------------|-----------------------------------------|------------------------------------------|-------------|
|            |                                         | CuSO <sub>4</sub> .                      | K. bichrom. |
|            |                                         | ° C.                                     | ° C.        |
| 12·0 noon  | Full sun, top and bottom .....          | 34·0                                     | 31·0        |
| 12.15 P.M. | Heavy clouds, otherwise same .....      | 23·0                                     | 21·0        |
| 12.30 „    | „ „ „ „ .....                           | 20·5                                     | 19·0        |
| 12.35 „    | „ „ „ „ .....                           | 19·0                                     | 18·0        |
| 1.0 „      | Full sun. „ Removed top screen .....    | 29·0                                     | 34·0        |
| 1.5 „      | Heavy clouds, otherwise same .....      | 23·0                                     | 23·5        |
| 1.10 „     | „ „ „ „ .....                           | 21·0                                     | 21·0        |
| 2.45 „     | „ „ „ „ .....                           | 16·0                                     | 16·25       |
| 5.0 „      | Raining „ „ „ .....                     | 16·5                                     | 16·5        |
| Next day—  |                                         |                                          |             |
| 9.0 A.M.   | Sun coming up „ „ .....                 | 14·5                                     | 15·0        |
| 1.0 P.M.   | Dull, rainy „ „ .....                   | 15·5                                     | 16·0        |
| Next day—  |                                         |                                          |             |
| 9.0 A.M.   | Sun not up, blue sky .....              | 17·25                                    | 17·5        |
| 9.15 „     | „ „ „ „ .....                           | 17·0                                     | 17·0        |
| 9.30 „     | „ „ „ hazy „ .....                      | 17·0                                     | 17·25       |
| 10.30 „    | Sun coming, but hazy.....               | 17·75                                    | 18·5        |
| 12.30 P.M. | Clouds and blue sky, sunny .....        | 18·5                                     | 20·5        |
| 1.0 „      | Clouds and bright sun, but not on ..... | 21·5                                     | 21·0        |
| 4.15 „     | Cloudy, blue sky.....                   | 20·25                                    | 19·25       |
| Next day—  |                                         |                                          |             |
| 11.30 A.M. | Cloudy, bright blue intervals .....     | 16·0                                     | 17·0        |
| 12.15 P.M. | Bright hot sun and clouds.....          | 21·0                                     | 21·0        |
| 1.0 „      | „ „ „ „ .....                           | 19·0                                     | 20·0        |

Before leaving this part of the subject, there is one other criticism to be taken into account. Experiments show that great care must be exercised with respect to the cooling effects of radiation from the thin coverslips supporting the hanging drops. I shall have occasion to illustrate this as we proceed; it is not so much that one does not recognise a danger of this kind, as that one does not know the magnitude of its effects until experiments have been made to determine them, that makes it worth while to note it particularly.

As we shall see, the danger does not depend entirely on the direct effects of the fall of temperature on the organism in the drop, but also on the dilution of the drop by condensation of water, and sometimes making it spread over the surface of the glass, and so on. These, and difficulties of similar kind, have given much trouble, and sometimes resulted in the ruin of experiments which promised useful results.

*Measurements of Growth in Light and Dark, under Known Conditions as to Temperature.*

I now proceeded to make experimental cultures with due regard to the information previously obtained, and especially taking care to check the temperatures by keeping control cells—which I now have expressly made with small, black-bulb thermometers in them. Moreover, as time went on, and experience increased, I discarded the use of glass as much as possible, and eventually succeeded in getting rid of it altogether; for the whole tendency of experience in these researches has been to show that the more glass the light rays have to traverse, the more difficult it is to trace their effect.

At first, however, I had to be content with using open windows and quartz floors to the cells, the light rays being reflected from the glass mirror of the microscope and passed through glass or other screens, &c. Subsequent developments will be described as I proceed.

In the first set of examples of experiments it will be noted that the exposures are from the beginning—i.e., it is the *spores* themselves which are submitted to the light-action; subsequently, I give experiments where the spores are first allowed to germinate out, and it is the actively growing *rods* or *filaments* which are exposed during post-germinal life.

*Experiments with Spores.*

Spores were sown as usual at 9 A.M., in broth drops, in cells with quartz floors, and the two cultures kept in the dark at 15° C. while further preparations proceeded.

By 10.15 A.M. four microscopes were fitted each with two flat

bottle-screens, filled with saturated alum solution, and a pair of coloured glasses, arranged as follows :—

On one microscope-stage lay an alum-screen over the mirror, and on this transparent screen a blue glass, then the culture chamber. Over the culture came another blue glass, and, lastly, an alum-screen.

Thus all light which passed into the chamber, whether reflected up from the mirror or direct from above, must traverse the alum-screen as well as the blue glass.

By the side of this stood a second microscope loaded in exactly the same way, except that a "dummy" cell with its (blackened bulb) thermometer replaced the true culture cell.

The third microscope was exactly as the first, but with red glasses in place of blue; and by its side an exactly similar control with a "dummy" cell.

Each pair of microscopes was further provided with a suspended thermometer, with bulb blackened, to give the air temperature.

All four microscopes stood side by side at the south window, and each was supplied with a narrow vertical shutter just broad enough to cast a band of shade across the mirror and loads, and so keep direct rays from the cells.

The day was brilliantly sunny, with deep-blue sky, and very few clouds till 2 P.M. Then heavy white clouds kept coming up, but the sunny intervals were numerous.

The following were the thermometric observations, the cultures (mirrors and screens) being illuminated *only by light from the sky*—never direct from the sun in the case of the cultures, and only occasionally and for short periods to get experimental information from the "dummy" cells.

From this table we observe (1) that the culture-cells were never allowed to exceed *at most* 25° to 26° C—indeed the cultures themselves never went so high, because the sun was not allowed to shine directly on either the mirrors or the top glasses; and (2) that, on the whole, the temperature of the cell between the red screens was about 0·5° to 1·0° higher than that of the cell between the blue screens.

| Time.       | Blue.                                                           |                         | Red.                                                        |                  |
|-------------|-----------------------------------------------------------------|-------------------------|-------------------------------------------------------------|------------------|
|             | Therm. in air.                                                  | Therm. in dummy.        | Therm. in air.                                              | Therm. in dummy. |
|             | ° C.                                                            | ° C.                    | ° C.                                                        | ° C.             |
| *10.15 A.M. | 17.5                                                            | 18.75                   | 17.5                                                        | 19.0             |
| 10.30 "     | 19.5 { Ray of sunlight                                          | 18.5                    | 16.0                                                        | 18.75            |
| †11.25 "    | 18.0                                                            | 18.0                    | 23.5 { Full sun on for a minute                             | 21.0             |
| 12 (noon)   | 20.0                                                            | 24.5 { Sun on top glass | 21.0                                                        | 24.5             |
| 12.30 P.M.  | 21.0                                                            | 26.0                    | 21.0                                                        | 27.0             |
| 12.40 "     | 23.0 { Brilliant light from sky near sun, not direct solar rays | 24.0                    | 24.0 { Brilliant light from sky near sun, but no direct sun | 26.0             |
| 12.50 "     | 24.0                                                            | 24.0                    | 21.75                                                       | 25.0             |
| ‡ 1.50 "    | 24.0                                                            | 26.0                    | 21.0                                                        | 27.0             |
| 2.25 "      | 19.5                                                            | 22.5                    | 18.0                                                        | 22.5             |
| 2.30 "      | 19.5                                                            | 22.5                    | 18.5                                                        | 22.5             |
| 2.38 "      | 19.0                                                            | 22.0                    | 18.5                                                        | 23.0             |
| 3.0 "       | 19.5                                                            | 20.0                    | 18.0                                                        | 20.0             |
| 4.15 "      | 19.0                                                            | 17.0                    | 18.0                                                        | 16.0             |
| 5.15 "      | 18.0                                                            | 16.0                    | 18.0                                                        | 16.0             |
| 9.0 "       | 16.0                                                            | 14.0                    | 16.0                                                        | 14.0             |

I examined the culture-drops at 12.30, 3 o'clock, 5.15 P.M., 9 P.M., and 11 P.M., taking note of the extremes and of the commonest lengths of the germinal filaments developed, with the following results (Curves 28 and 29, p. 337) :—

That the light and not the temperature was the effective agent in delaying the germination of the spores between the blue glasses hardly admits of any doubt after this experiment, the most astounding thing about the matter being the prolonged action of the inhibition effect; for it was permissible to expect that when once the germination had begun, the plantlets would rapidly gain strength during the dark hours of the night. Such was not the case, however, and this fact certainly seems to support the idea that the action is on something—whether substance or machinery—in the spore itself, so that the plant produced is really a weakened organism incapable of assimilating the nutritive substances of the food-medium with anything like the normal vigour.

Later on, it is true, the retarded blue culture did slowly recover

\* Opened window for three minutes = all fell 5 to 6° C., but rose on closing again.

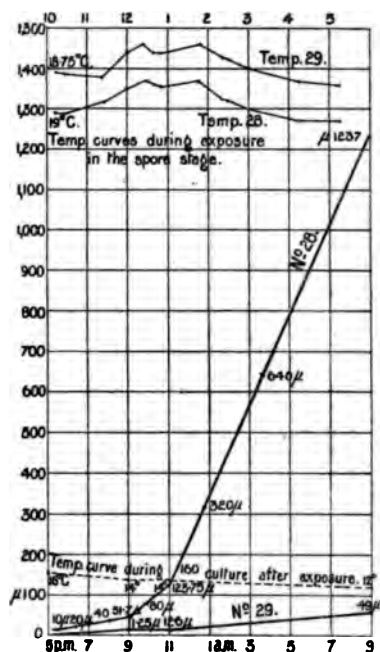
† On removing shutters from both dummies and turning mirror to sun, the readings during consecutive minutes = 19, 21, 25, 28, 31 (for blue), and 24, 27, 31, 35, 38 (for red); they fell as rapidly on replacing shutter and taking light from blue sky only.

‡ On exposing full as possible to sun the red dummy rose to 40—41.5° in a few minutes, the blue dummy to 39—40°. They fell as quickly on replacing. When fully shaded from all sunlight, both fell, in five minutes, to 22.5°.

## Germination behind Coloured Screens.

| Time.      | Blue.      |                                 |                                 |                   |  | Red.       |                                 |                                 |                   |  |
|------------|------------|---------------------------------|---------------------------------|-------------------|--|------------|---------------------------------|---------------------------------|-------------------|--|
|            | Average.   | Extremes.                       | Mean.                           | Commonest length. |  | Average.   | Extremes.                       | Mean.                           | Commonest length. |  |
| 12.30 P.M. | $\mu$ .    | $\mu$ .<br>Spores swollen only. | $\mu$ .<br>Spores swollen only. | $\mu$ .           |  | $\mu$ .    | $\mu$ .<br>Spores swollen only. | $\mu$ .<br>Spores swollen only. | $\mu$ .           |  |
| 3 "        |            | No germination.                 |                                 |                   |  |            |                                 |                                 |                   |  |
| 5.15 "     |            | None germinated out.            |                                 |                   |  |            |                                 |                                 |                   |  |
| 9 "        | 10.4 (5)   | 9-13.5                          | 11.25                           | 9                 |  | 10 (3)     | 9-11.25                         | 10                              | 9-11.25           |  |
| 11 "       | 10.5 (13)  | 9-16.25                         | 12.6                            | 9-10              |  | 51.7 (14)  | 13.5-90                         | 51.7                            | 45-65             |  |
| 9 A.M.     | 38.82 (10) | 9-90                            | 49                              | 40                |  | 112.8 (12) | 22.5-225                        | 123.75                          | 112.5             |  |
|            |            |                                 |                                 |                   |  | —          | 675.0-1800                      | 1287                            | —                 |  |

Curves 28 and 29.



and eventually grew into normal and rapidly-spreading filaments; but the effects of the retardation and weakness were evident in the poverty and lateness of its spore-formation.

I repeated the foregoing experiment the next day, which was also sunny and with a blue sky with passing white clouds, under exactly the same conditions, excepting that the general temperature was lower throughout and the sky less clear. The utmost care was taken in the arrangements of the shutters, positions of microscope mirrors, &c., and no direct sunlight touched any of the apparatus. The thermometer readings were as follows—all with bulbs blackened:—

The readings show (1) that the temperature varied very slightly, and (2) that of the red culture was a trifle higher than that of the blue.

The spores, which had been sown at 10 A.M., and exposed at 10.30, were examined from time to time as before; and, also as before, germination began in the red culture long before it did in the blue.

After exposure for the day, I placed the cultures from 11 P.M. near an open window, with a pair of maximum and minimum thermometers by their side, in order to check the growth during the night, because there was a danger of the red one forming filaments

| Time.                                               | Blue. |       | Red.  |       |
|-----------------------------------------------------|-------|-------|-------|-------|
|                                                     | Air.  | Cell. | Air.  | Cell. |
|                                                     | ° C.  | ° C.  | ° C.  | ° C.  |
| 10.33 A.M.                                          | 16·0  | 14·0  | 16·0  | 14·0  |
| 11.30 "                                             | 18·0  | 16·0  | 18·0  | 16·0  |
| 11.45 "                                             | 16·5  | 16·0  | 18·0  | 17·0  |
| 12 (noon)                                           | 16·0  | 15·0  | 17·0  | 16·0  |
| 12.15 P.M.                                          | 17·0  | 15·0  | 18·0  | 15·5  |
| 12.30 "                                             | 18·0  | 16·5  | 19·5  | 16·5  |
| 12.45 "                                             | 19·0  | 17·0  | 19·0  | 17·0  |
| 1.0 "                                               | 18·25 | 17·0  | 19·0  | 17·0  |
| 2.0 "                                               | 18·5  | 16·0  | 17·75 | 16·0  |
| 3.0 "                                               | 19·0  | 17·5  | 19·0  | 17·5  |
| 3.30 "                                              | 19·0  | 17·0  | 18·0  | 17·5  |
| 6.0 "                                               | 16·0  | 15·0  | 16·75 | 16·0  |
| 9.0 "                                               | 15·0  | 13·0  | 15·5  | 14·0  |
| 11.0 "                                              | 14·0  | 12·5  | 14·5  | 13·5  |
| 9.0 A.M.                                            | 14·0  | 7·0   | 14·0  | 7·0   |
| Changed red glass for blue, and <i>vice versâ</i> . |       |       |       |       |
| 10.15 A.M.                                          | 14·25 | 13·0  | 14·0  | 12·5  |
| 11.15 "                                             | 16·0  | 16·0  | 15·75 | 14·5  |
| 1.0 P.M.                                            | 19·5  | 18·0  | 19·5  | 18·0  |
| 2.0 "                                               | 18·5  | 18·75 | 20·0  | 18·75 |
| 5.0 "                                               | 18·0  | 15·75 | 19·0  | 16·25 |

longer than I could measure next morning. The device was successful, as the curve shows.

Then, having measured both cultures at 9 A.M. next morning, I placed the one hitherto between *red* glasses between the *blue* glasses, and *vice versâ*, to see if, during the second day's exposure, there would be any appreciable changes in the curves. The results are given in the accompanying table and curves (Curves 30 and 31):—

As before, the germination in the blue was markedly retarded, as shown by the measurements up to 11 P.M. Both grew much more slowly during the night (when the temperature fell to 7° C.) than would have been the case at 12—14° C. (see preceding curve); and both began to grow quickly after 9 A.M., in accordance with the rapid rise of temperature from 9 to 11 A.M.

But the most interesting result is shown in the growth from 11 A.M. onwards, when the light began to increase in intensity, for the culture hitherto under blue glass, but now under *red*, began to grow much more rapidly, so that its curve offered to cross that of the now *retarded* culture previously in the red and now in the *blue*.

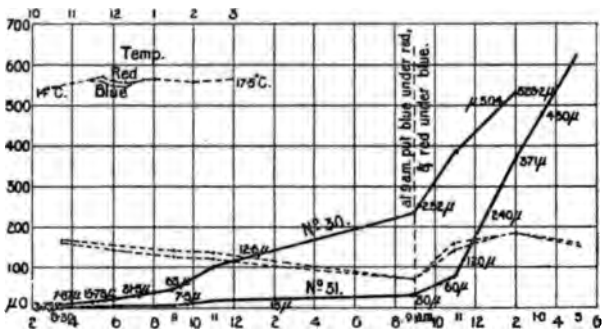
It seemed almost impossible to deny that this must be proof of the light-action.

Spores were sown at 10 A.M. in water, with one drop of broth per

## Germination behind Coloured Screens.

| Time.                                       | Blue.                                |              |              |                    | Red.                                                   |              |              |                    |
|---------------------------------------------|--------------------------------------|--------------|--------------|--------------------|--------------------------------------------------------|--------------|--------------|--------------------|
|                                             | Average.                             | Extremes.    | Mean.        | Commonest lengths. | Average.                                               | Extremes.    | Mean.        | Commonest lengths. |
| 2.0 P.M.                                    | $\mu$ .<br>No traces of germination. | $\mu$ .<br>— | $\mu$ .<br>— | $\mu$ .<br>—       | $\mu$ .<br>Spores swelling and beginning to germinate. | $\mu$ .<br>— | $\mu$ .<br>— | $\mu$ .<br>—       |
| 3.30 "                                      | 3.75 (10)                            | 3.0 — 4.5    | 3.75         | 3—4                | 7.65 (10)                                              | 6.75— 9.0    | 7.87         | 7—8                |
| 6.0 "                                       | 4.5 (10)                             | 4.0 — 5.0    | 4.5          | 4.5                | 20.14 (20)                                             | 13.5 — 29.25 | 21.3         | 22.5               |
| 9.0 "                                       | 6.0 (11)                             | 5.0 — 8.0    | 6.5          | 5.5                | 51.75 (10)                                             | 22.5 — 81.0  | 51.7         | 45.0               |
| 11.0 "                                      | 10.4 (12)                            | 6.75—15.0    | 10.8         | 11.25              | 96.7 (15)                                              | 45.0 —157.5  | 101.25       | 100.0              |
| 9.0 A.M.                                    | 40.0 (11)                            | 9.0—54.0     | 31.0         | 45.0               | 210.0 (8)                                              | 112.5 —360.0 | 236.0        | 160—180            |
| Changed red glass for blue, and vice versa. |                                      |              |              |                    |                                                        |              |              |                    |
| 11.0 A.M.                                   | 86.7 (13)                            | 40.5—131.0   | 80.5         | 90.0               | 374.0 (13)                                             | 245.0 —540.0 | 392.0        | 360.0              |
| 2.0 P.M.                                    | 324.0 (10)                           | 202.5—540.0  | 371.0        | 290.0              | 466.5 (8)                                              | 247.5 —810.0 | 528.2        | 450.0              |
| 5.0 "                                       | 652.5 (5)                            | 337.0—900.0  | 618.0        | 675.0              | —                                                      | —            | —            | —                  |

Curves 30 and 31.



100 c.c., and exposed forthwith, one behind  $\text{CuSO}_4$ , the other behind K. bichromate.

No direct sunlight was allowed to fall on the cultures, cardboard shutters being used, and the mirrors took the light from the sky only. The day was hot and sunny, with heavy clouds occasionally and dull rainy afternoon, the following being the temperature, &c., records.

The cultures were examined at 2 P.M., 5 P.M., 10 P.M., and 9 A.M. next morning, with results as given below :—

| Time.                                                 | Bichromate.<br>$t = \text{cell.}$ | $\text{CuSO}_4$ .<br>$t = \text{cell.}$ |
|-------------------------------------------------------|-----------------------------------|-----------------------------------------|
|                                                       | ° C.                              | ° C.                                    |
| 10.0 A.M.                                             | 15.0                              | 14.5                                    |
| 11.30 "                                               | 22.0                              | 21.0                                    |
| 12.15 P.M.                                            | 20.0                              | 19.0                                    |
| At 12.30 turned full sunlight on and rose<br>at once  | 32.0                              | 25.0                                    |
| Falling in 5 minutes, on readjusting<br>shutters, &c. | 25.0                              | 22.0                                    |
| 1.0 P.M.                                              | 20.0                              | 19.0                                    |
| 2.0 "                                                 | 23.0                              | 22.0                                    |
| On turning full sunlight on, runs up at<br>once       | 30.0                              | 25.0                                    |
| Falls in 5 minutes, on readjusting                    | 24.5                              | 23.5                                    |
| 5.0 P.M.                                              | 16.0                              | 17.0                                    |
| 10.0 "                                                | 13.5                              | 12.5                                    |
| 9.0 P.M.                                              | 13.5                              | 13.0                                    |

No germination had occurred in either culture at 2 P.M., nor even at 5 P.M.; but at 10 P.M. both had germinated, and the condition of affairs was as follows :—

In the blue culture the longest rods were  $17.6 \mu$ , and the shortest  $8.8 \mu$ , giving a mean of  $13.2$ , which was about the average length.

In the red culture, however, I found filaments  $44 \mu$ ,  $76 \mu$ ,  $110 \mu$ , and even  $150 \mu$  in length. Mean of extremes =  $97 \mu$ .

On leaving the cultures through the night, at about 12—13° C., and examining at 9 A.M. next day, the one over bichromate had filaments from 220  $\mu$  to 660  $\mu$  long, whereas none in the blue light were more than 88  $\mu$  to 130  $\mu$  long.

Since the variations in temperature (N.B.—The rapid and extreme variations only refer to the control cells, and were made for experimental purposes) only concern the exposure of the, as yet, ungerminated spores, we can hardly err in referring the retardation of the spores in the blue-light to the inhibitory action of the light rays.

At 7 A.M. I sowed spores in gelatine-broth, and exposed forthwith at an east window. The cells had a quartz floor, and a plate of glass covered with tin-foil was put over them to keep the sun off the top glass. One was over red glass, the other over blue, and neither received any but rays reflected through these glasses from the microscope mirrors.

Each had a dummy cell by its side, arranged similarly, the temperatures of which ran as follows:—

| Time.     | Blue.<br>° C. | Red.<br>° C. |
|-----------|---------------|--------------|
| 7.0 A.M.  | 17.5          | 17.5         |
| 9.15 "    | 20            | 20           |
| 9.45 "    | 22.5          | 22.5         |
| 10.20 "   | 24            | 24           |
| 12 noon   | 23.5          | 22           |
| 3.45 P.M. | 20.5          | 20.5         |

The day was bright, and every care was taken to keep the direct sun's rays off by means of shutters, &c.

At 4 P.M. both cultures were removed to the south window, where the temperature was 21° C., and kept so till near 7 P.M., at which hour it had fallen to 20° C. It slowly fell during the night, as shown by curve, to 19—21°.

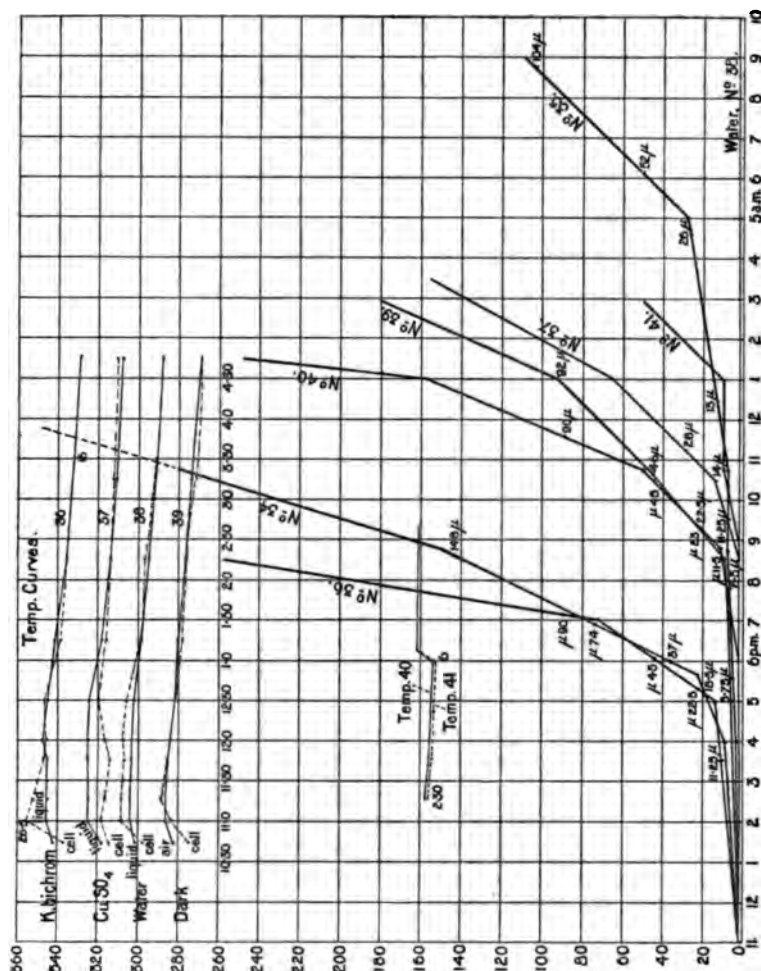
The table on the next page gives the results (see also Curves 34 and 35, p. 343):—

Here, again, the results seem definitely positive, and only to be accounted for as due to the light rays, and one must conclude that—with resting spores at any rate—the above exposure to reflected light through blue glass causes very pronounced retardation, suggesting that a little longer exposure (or a somewhat more intense light) would have inhibited the germination entirely.

| Time.   | Red.                                                     |                                    |         |            | Blue.    |           |         |            |
|---------|----------------------------------------------------------|------------------------------------|---------|------------|----------|-----------|---------|------------|
|         | Average.                                                 | Extremes.                          | Mean.   | Commonest. | Average. | Extremes. | Mean.   | Commonest. |
| 7 A.M.  | $\mu$ .                                                  | $\mu$ .                            | $\mu$ . | $\mu$ .    | $\mu$ .  | $\mu$ .   | $\mu$ . | $\mu$ .    |
| 12 noon |                                                          | Sown.                              |         |            |          | Sown.     |         |            |
| 4 P.M.  | 9 (5)                                                    | 4.5-14                             | 9.25    | 9-10       | 2.5 (5)  | 2-4.5     | 3.25    | 2-3        |
| 5.40 "  | 23 4 (7)                                                 | 9-35                               | 22      | 20-25      | 6.4 (6)  | 4.5-5     | 4.75    | 4-5        |
| 8.45 "  | 150 (4)                                                  | 100-200                            | 150     | 150        | 7 (4)    | 5-9       | 7       | 7          |
| 10.45 " | 300 (5)                                                  | 200-400                            | 300     | 300        | 8.5 (4)  | 6-10      | 8       | 8          |
| 5 A.M.  | Too                                                      | long to measure, over 1000 $\mu$ . |         |            | 20 (8)   | 10-45     | 27.5    | 20         |
| 11 "    | Dense meshwork of filaments several thousand $\mu$ long. |                                    |         |            | 200 (6)  | 80-600    | 340     | 150*       |

\* There was one filament about 500 to 600  $\mu$  long, and this brought up the average, but the majority were not above 150  $\mu$  or so.

Curves 34 and 35.



### *Effect of Light from Blue Sky on Spores.*

I now made a new departure. Starting from the fear lest any heating up of the bichromate, &c., screens, and the passage of the solar rays through so many glass layers, &c., might affect the results, I arranged screens and cultures as follows:—

Three large circular crystallisation dishes, 12 in. wide by 4 in. deep, were taken; they were of thin clear glass, with vertical walls and flat floor. These were supported, bottom down, each on four corks just large enough to clear the cover-slip roofs of my cells, with their hanging drops.

One dish was filled to a depth of 3 in. with clear water; one with  $2\frac{1}{2}$  in. of  $\text{CuSO}_4\text{Am}$ , and the other with  $2\frac{1}{2}$  in. K. bichromate.

Under each of the two latter I placed a plane mirror, flat, on which I laid the culture cell (quartz floor), and one of the "dummy cells" with its black-bulb thermometer. Under the water dish I placed a culture and its "dummy" control on a piece of slate; and by the side of this a culture and its dummy under a bell-jar darkened with foil and brown paper. Under the bell-jar I also hung a thermometer in the air. All four pieces of apparatus were on a large box outside the laboratory and looking east and north.

The day opened with a clear blue sky and light wind, but a good deal of white cloud (cumulus) came over after 10 A.M., and there was some haze about 1 P.M. After 2 P.M. a magnificent clear blue sky. The sun was on the box until noon, but it was often covered by white clouds, especially from 11.45 to 12.15 or so.

The following table (p. 345) gives the temperatures.

The sowings were made in broth-gelatine drops about 10.30 A.M., and were at once exposed as described. The dishes were so large that no side light could get in, and I was certain that only the light from the sky, after traversing the liquid, glass-bottom of dish, and thin cover-slip, reached the drop.

Each dish had a thermometer in the liquid, and care was taken\* to avoid any complications arising from the cusp of light reflected from the vertical walls.

In order to meet the objection that the orange and blue liquids and cultures varied so much in temperature during the first  $1\frac{1}{2}$  hours, I made two new sowings at 2.30, and put one under the blue and the other under the orange dish, exactly as before, and left them there till 6 P.M., so that they got  $3\frac{1}{2}$  hours' exposure to the open sky only—no direct sun. The retardation in the blue is, nevertheless, nearly as marked as in the other cultures.

\* After my attention was directed to the danger by noticing the high temperature to which the bichromate dummy cell was running at 11 A.M.; only the *dummy* was being touched by the cusp, however, so that the temperature recorded for the actual culture cell at this moment is too high.

## Temperatures.

| Time.                         | Dark culture. |       | Water.  |       | CuSO <sub>4</sub> Am. |       | K. Bichromate. |       |
|-------------------------------|---------------|-------|---------|-------|-----------------------|-------|----------------|-------|
|                               | Air.          | Cell. | Liquid. | Cell. | Liquid.               | Cell. | Liquid.        | Cell. |
| 10.40 A.M.                    | ° C.          | ° C.  | ° C.    | ° C.  | ° C.                  | ° C.  | ° C.           | ° C.  |
| 11.0 "                        | 21.0          | 17.5  | 20.5    | 19.0  | 19.0                  | 17.5  | 20.5           | 19.0  |
| 11.15 "                       | 23.0          | 21.5  | 22.0    | 24.0  | 22.0                  | 19.0  | 22.5           | 28.5  |
| 11.45 "                       | 22.0          | 22.0  | 21.5    | 23.0  | 22.5                  | 18.25 | 22.5           | 27.0  |
| 12 noon                       | 21.5          | 21.5  | 21.5    | 23.5  | 22.5                  | 17.5  | 22.5           | 23.5  |
| 12.30 P.M.                    | 20.5          | 20.5  | 19.5    | 22.5  | 22.5                  | 18.5  | 23.5           | 23.0  |
| 1.15 "                        | 20.0          | 19.5  | 19.5    | 22.5  | 22.0                  | 20.0  | 22.5           | 23.0  |
| 2.15 "                        | 18.5          | 18.5  | 18.0    | 19.5  | 19.0                  | 18.0  | 20.5           | 19.0  |
| 3.30 "                        | 17.0          | 16.0  | 16.0    | 18.5  | 17.0                  | 17.0  | 18.0           | 18.0  |
| 4.0 "                         | —             | —     | —       | 21.0  | 15.0                  | 15.5  | 16.0           | 16.0  |
| 10.30 "                       | —             | 20.5  | —       | 20.5  | —                     | 21.0  | —              | 21.0  |
| 1.0 A.M.                      | —             | 19.5  | —       | 19.5  | —                     | 20.5  | —              | 20.5  |
|                               |               |       |         |       |                       | 19.5  | —              | 19.5  |
| Temperature of 2.30 Cultures. |               |       |         |       |                       |       |                |       |
| 2.30 "                        | —             | —     | —       | —     | 17.0                  | 17.0  | 18.0           | 18.0  |
| 3.30 "                        | —             | —     | —       | —     | 15.0                  | 15.5  | 16.0           | 16.0  |
| 4.45 "                        | —             | —     | —       | —     | 14.0                  | 15.0  | 15.0           | 15.0  |
| 6.0 "                         | —             | —     | —       | —     | 13.0                  | 13.25 | 14.5           | 14.0  |
| 8.45 "                        | —             | —     | —       | —     | —                     | 21.0  | —              | 21.0  |
| 10.45 "                       | —             | —     | —       | —     | —                     | 20.5  | —              | 20.5  |
| 1.0 "                         | —             | —     | —       | —     | —                     | 19.5  | —              | 19.5  |

At 3.30 all the four cultures were brought into the laboratory and placed side by side under a dark cover, and allowed to grow through the night at 21° C., falling to 20.5° at 10.30 P.M., and to 19.5° at 1 A.M., where it remained until 9 A.M., when the last observations were made.

The two 2.30 cultures were brought in at 6 P.M., and remained side by side with the others through the night.

If we examine these results (Curves 36—41, p. 343), the striking effects of the bichromate come out more clearly than ever. It is an undeniable fact that the spores germinate more quickly, produce stronger and more rapidly developing filaments, and sporify more early after exposure to the light coming through the orange screens than when left in the dark or behind any of the other screens for the same period.

Why is this? The reply first suggested is, because the bichromate lets so much of the heat rays through that the spores are raised to a higher temperature. But the same acceleration of germination is seen if the bichromate and  $\text{CuSO}_4$  screens are not exposed to the sun, but only to light from the blue sky, and when the temperature of the cells behind them hardly differs—at any rate not more than

| Time.      | Red.                                                                   |            |        |                 | Blue.                                          |           |       |                                     |
|------------|------------------------------------------------------------------------|------------|--------|-----------------|------------------------------------------------|-----------|-------|-------------------------------------|
|            | Average.                                                               | Extremes.  | Mean.  | Com-<br>monest. | Average.                                       | Extremes. | Mean. | Com-<br>monest.                     |
| 3.30 P.M.* | 11.25 (6)                                                              | 9—13.5     | 11.25  | 9—10            | μ.                                             | μ.        | μ.    | μ.                                  |
| 5.0 "      | 16.0 (10)                                                              | 9—18       | 14.5   | 9—12            | No germination.                                |           |       |                                     |
| 7.0 "      | 90.45 (10)                                                             | 31.5—112.5 | 72.0   | 90              | "                                              |           |       |                                     |
| 8.30 "     | 258.75 (10)                                                            | 157—360    | 258.0  | 225—250         | "                                              |           |       |                                     |
| 10.30 "    | 1000.0 (10)                                                            | 900—1150   | 1025.0 | 950             | 9.1 (10)                                       | 4.5—23.75 | 14.0  | Most are still ungerminated to 5 μ. |
| 1.0 A.M.   | Immeasurable; dense coils of filaments, certainly 2000 μ and more long |            |        |                 | 33.0 (16)                                      | 9—121.5   | 65.25 | 12—15                               |
| 9.0 "      | " "                                                                    |            |        |                 | Cannot measure, but most = about 450 to 900 μ. |           |       |                                     |
| 8.45 P.M.† | 10.6 (6)                                                               | 9—13.5     | 11.25  | 9—10            | Not yet germinated.                            |           |       |                                     |
| 10.45 "    | 45.0 (14)                                                              | 27—72      | 49.5   | 35—40           | 10.0 (2)                                       | 4.5—15.75 | 10.0  | Could only find two germinated 9    |
| 1.0 A.M.   | 160.0 (10)                                                             | 45—270     | 157.5  | 150—160         | 10.8 (7)                                       | 6.75—13.5 | 10.0  | Could only estimate 450—900         |
| 9.0 "      | Immeasurable; i.e., filaments 1000 μ or more                           |            |        |                 | —                                              | —         | —     |                                     |

\* Sown at 10.30 A.M.

† Sown at 2.30 P.M.

a degree. Moreover, the temperature of the water-cell also rose considerably above that of the  $\text{CuSO}_4$  cell. Why, then, did the spores exposed to light through water not germinate out more rapidly? Those in the *dark* did! though not so rapidly as those behind bichromate.

It seems to me the only possible explanation is that behind the bichromate screen we have the effects of a higher temperature—due to the transmitted infra-red rays—superposed on the beneficial effects of the absence of the inhibiting blue rays, which the bichromate cuts off.

I think the absorbed infra-red rays must be effective in some way, or else the dark culture ought to have germinated as readily. If this is so, it raises an extremely interesting and important question as to the sources of energy available from without to these organisms, and may possibly help to throw light on many obscure points connected with the extraordinary capacity for work which they show.

But we cannot explain the striking difference between the water and the bichromate action as merely due to temperature. Indeed none of the results would accord with that, for it was merely a ques-

| Water.   |                           |         |            | Dark.     |                                                   |         |            |
|----------|---------------------------|---------|------------|-----------|---------------------------------------------------|---------|------------|
| Average. | Extremes.                 | Mean.   | Commonest. | Average.  | Extremes.                                         | Mean.   | Commonest. |
| $\mu$ .  | $\mu$ .                   | $\mu$ . | $\mu$ .    | $\mu$ .   | $\mu$ .                                           | $\mu$ . | $\mu$ .    |
|          | No germination            |         |            |           | No germination.                                   |         |            |
|          | "                         |         |            | 5·6 (10)  | 4·5—6·75                                          | 5·75    | 4·5        |
|          | "                         |         |            | 6·6 (10)  | 5—9                                               | 7·0     | 5·0        |
|          | "                         |         |            | 9·9 (10)  | 5—13·5                                            | 9·25    | 9·0        |
|          | "                         |         |            | 31·5 (11) | 18—67·5                                           | 42·75   | 22·5       |
|          | "                         |         |            | 72·0 (12) | 36—112                                            | 74·0    | 45—50      |
|          | None germinated—all dead. |         |            |           | Dense meshwork of filaments over 1000 $\mu$ long. |         |            |

Curves 36-41.  
2 B 2

tion of the rise in temperature between 10.40 and 12.30, before germination had begun be it remembered, and therefore acting on spores which will bear a high temperature, *e.g.*, 60° C. for some hours. The order of germination ought to have been (1) bichromate, (2) water, (3) dark, and (4)  $\text{CuSO}_4$  culture, whereas it was (1) bichromate, (2) dark, (3)  $\text{CuSO}_4$ , and the spores behind water did not germinate at all, but were there lying dead.

In order to settle the doubt as to temperature, or sudden slight alterations of temperature, being the cause of the retardation and death of the spores, I repeated the experiment on the following day, exactly as before, excepting with the single modification that the table was so placed that *no direct sunshine whatever* was allowed to fall on the apparatus or spores; they received no light except that from the blue sky and white cumulus clouds passing through the liquids and glass.

The results, summarised in the table and curves (pp. 350, 351, and 353), show that my conclusion was right; the effect is solely an effect of the light passing through the screens.

The accompanying table (p. 349) gives the temperatures, during exposure, of the liquids, the air in the dark bell-jar, and the cells. At 4 p.m. the cells were all brought into the laboratory and kept under a dark jar. The temperatures during the night and following day are also recorded. The differences in temperature are so slight that the spores may be regarded as exposed to the same conditions as regards heat. Nevertheless the red and dark cultures germinated out five or six hours before the blue, and were much stronger and more rapid in growth, while the spores under water were killed, and all this with only five hours' exposure to diffuse light from a blue sky, which was, moreover, slightly hazy for a considerable part of the time, and never entirely free from white clouds.

This experiment seems conclusive. Moreover, it is instructive in other respects. It shows, for instance, that when the dark heat rays (transmitted by the bichromate the day before) are absent, the bichromate culture germinates out and grows like that in the dark, and that these rays are not necessary for the bactericidal effect through water. That the culture behind the  $\text{CuSO}_4$  germinated out at all is no doubt due to the thick layer of strong  $\text{CuSO}_4$  diminishing the quantity and intensity of the light; it is pretty certainly a mere matter of length of exposure.

It seems to me the importance of these results can scarcely be over-estimated in their bearing on the question of the death of bacteria in water. As the sun shines for a longer time and with more intensity on the river day by day with the advance of spring to summer the spores will be more and more retarded, and even those which escape actual death with the advent of the night are again

## Temperatures.

| Time.     | Water. |         | Dark. |       | Red.  |         | Blue. |         |
|-----------|--------|---------|-------|-------|-------|---------|-------|---------|
|           | Cell.  | Liquid. | Cell. | Air.  | Cell. | Liquid. | Cell. | Liquid. |
| 11.0 A.M. | ° C.   | ° C.    | ° C.  | ° C.  | ° C.  | ° C.    | ° C.  | ° C.    |
| 11.30 "   | 16.5   | 16.0    | 16.5  | 16.5  | 16.75 | 16.0    | 16.25 | 15.0    |
| 12.0 noon | 17.0   | 17.0    | 15.5  | 16.5  | 15.5  | 15.5    | 17.0  | 17.0    |
| 1.0 P.M.  | 17.5   | 17.0    | 16.0  | 17.0  | 17.0  | 18.0    | 16.75 | 16.0    |
| 2.0 "     | 17.0   | 16.0    | 16.0  | 18.0  | 17.0  | 17.0    | 16.5  | 17.0    |
| 2.30 "    | 17.0   | 15.0    | 16.5  | 17.0  | 17.0  | 17.5    | 16.0  | 16.5    |
| 4.0 "     | 17.0   | 15.0    | 16.5  | 17.0  | 17.0  | 17.0    | 16.0  | 16.0    |
| 4.15 "    | 16.5   | 14.0    | 16.0  | 16.25 | 16.0  | 15.5    | 15.0  | 15.5    |
| 5.45 "    | 21.5   | —       | 21.5  | —     | 21.5  | —       | 21.5  | —       |
| 7.0 "     | 21.5   | —       | 21.5  | —     | 21.5  | —       | 21.5  | —       |
| 8.45 "    | 21.0   | —       | 21.0  | —     | 21.0  | —       | 21.0  | —       |
| 11.30 "   | 20.0   | —       | 20.0  | —     | 20.0  | —       | 20.0  | —       |
| 2.0 A.M.  | 19.0   | —       | 19.0  | —     | 19.0  | —       | 19.0  | —       |
| 6.30 "    | 18.0   | —       | 18.0  | —     | 18.0  | —       | 18.0  | —       |
| 10.0 "    | 16.0   | —       | 16.0  | —     | 16.0  | —       | 16.0  | —       |
| 2.0 P.M.  | 17.0   | —       | 17.0  | —     | 17.0  | —       | 17.0  | —       |
| 7.0 "     | 19.0   | —       | 19.0  | —     | 19.0  | —       | 19.0  | —       |
| 7.0 "     | 19.0   | —       | 19.0  | —     | 19.0  | —       | 19.0  | —       |

exposed a few hours later as the early dawn brings the light on them once more. So that even if it were true that the bacillus is safe as soon as it has germinated, it would still be the case that only those spores which lurk in the shadows, or are rolled by the river into unexposed places, could recover.

We shall see later on, however, that this action of light on the spores in water is favoured by the low temperatures to which the river organisms are usually subjected for subsequent experiments show very clearly that whatever resisting action they may be supposed to have is put in abeyance when the temperature is too low for active and rapid growth.

| Time.     | Water.           |          |       |             | Dark.                        |             |       |             |
|-----------|------------------|----------|-------|-------------|------------------------------|-------------|-------|-------------|
|           | Average.         | Extremes | Mean. | Com-monest. | Average.                     | Extremes.   | Mean. | Com-monest. |
|           | μ.               | μ.       | μ.    | μ.          | μ.                           | μ.          | μ.    | μ.          |
| 5.45 P.M. | None germinated. |          |       |             | None germinated.             |             |       |             |
| 7.0 "     | " "              |          |       |             | Germination beginning.       |             |       |             |
| 8.45 "    | " "              |          |       |             | 8.2 (10)                     | 4.5—11.25   | 7.9   | 6—7         |
| 1.30 "    | " "              |          |       |             | 40.5 (10)                    | 27.0—58.5   | 42.75 | 36          |
| 2.0 A.M.  | " "              |          |       |             | 158.85 (10)                  | 90.0—220.0  | 155.0 | 45—90       |
| 6.30 "    | " "              |          |       |             | 528.75 (4)                   | 200.0—900.0 | 550.0 | 450—600     |
| 0.0 "     | " "              |          |       |             | Immeasurably long coils, &c. |             |       |             |
| 2.0 P.M.  | " "              |          |       |             | "                            | "           | "     | "           |
| 7.0 "     | " "              |          |       |             | "                            | "           | "     | "           |

To test the germinating power of the spores after baking, or, rather, drying at high temperatures, I proceeded as follows:—

A number of cover-slip preparations were made in the ordinary way, with spores distributed in distilled sterile water, and these were placed, infected side upwards, in Petri dishes, and exposed to 80° C. in the hot air sterilizer, and allowed to cool slowly (two hours) to 30° C.

The results show clearly that the germinating power of these spores dried at high temperatures is by no means destroyed by the high temperatures to which they are subjected.

In the series selected the spores were distributed on the slips at 10 A.M., and at once put into the hot-air steriliser at 80° C. At 12 noon they had cooled to just below 30° C., and each slip received a drop of broth-gelatine, and was arranged as the roof to a cell, the culture being now a hanging drop.

I take three of them, labelled 1, 2, and D. Nos. 1 and 2 were at once put into the incubator at 21° C.; this was 12.15 P.M. D was kept dry in diffuse light at 20° till 4 P.M., and then put in as the others.

The following table (p. 352) summarises the result.

All things considered, and especially the difficulties of measuring the averages of the more advanced cultures, these three curves agree very well, and show clearly that neither the germination nor the subsequent growth of the bacillus is in the least hurt by the heating and drying of the spores. Indeed, it is difficult not to suspect that the vigour of the organism is *improved* rather than injured by the treatment.\*

#### Experiments with Growing Filaments.

Three broth-drop cultures were started; and placed at 20–22° C.

| Red.                     |           |       |             | Blue.                |           |       |             |
|--------------------------|-----------|-------|-------------|----------------------|-----------|-------|-------------|
| Average.                 | Extremes. | Mean. | Com-monest. | Average.             | Extremes. | Mean. | Com-monest. |
| μ.                       | μ.        | μ.    | μ.          | μ.                   | μ.        | μ.    | μ.          |
| None germinated.         |           |       |             | None germinated.     |           |       |             |
| Germination beginning.   |           |       |             |                      |           |       |             |
| 6·5 (10)                 | 4·5—9     | 6·25  | 5—6         | "                    | "         | "     | "           |
| 25·65 (10)               | 13·5—36   | 24·75 | 25          | "                    | "         | "     | "           |
| 111·6 (10)               | 59·0—157  | 108·0 | 45—90       | "                    | "         | "     | "           |
| 823·0 (3)                | 675·0—900 | 787·5 | 800—900     | 10·5 (10)            | 4·5—45    | 24·75 | 5           |
| Immeasurably long coils. |           |       |             | 60·0 (10)            | 15—112    | 63·5  | 25—45       |
| "                        | "         | "     | "           | 225·0 (10)           | 90—450    | 270·0 | 200         |
| "                        | "         | "     | "           | Too long to measure. |           |       |             |

Curves 42–45.

in the dark at 9 A.M., where they remained until 2 P.M., when germination had begun in all.

They were then distributed as follows:—One remained in the dark under the bell-jar covered with foil and paper; a second was over a bottle screen of water, tinged slightly blue with  $\text{CuSO}_4$ , and arranged so that no direct rays reached it—the top being shaded by a thick card; while the third was exactly as the second, except that its screen was of K. bichromate. The two cultures in the light had each a dummy cell and thermometer rigged up exactly in the same way, and these and thermometers by their side and in the bell-jar gave the temperatures of the air and cultures.

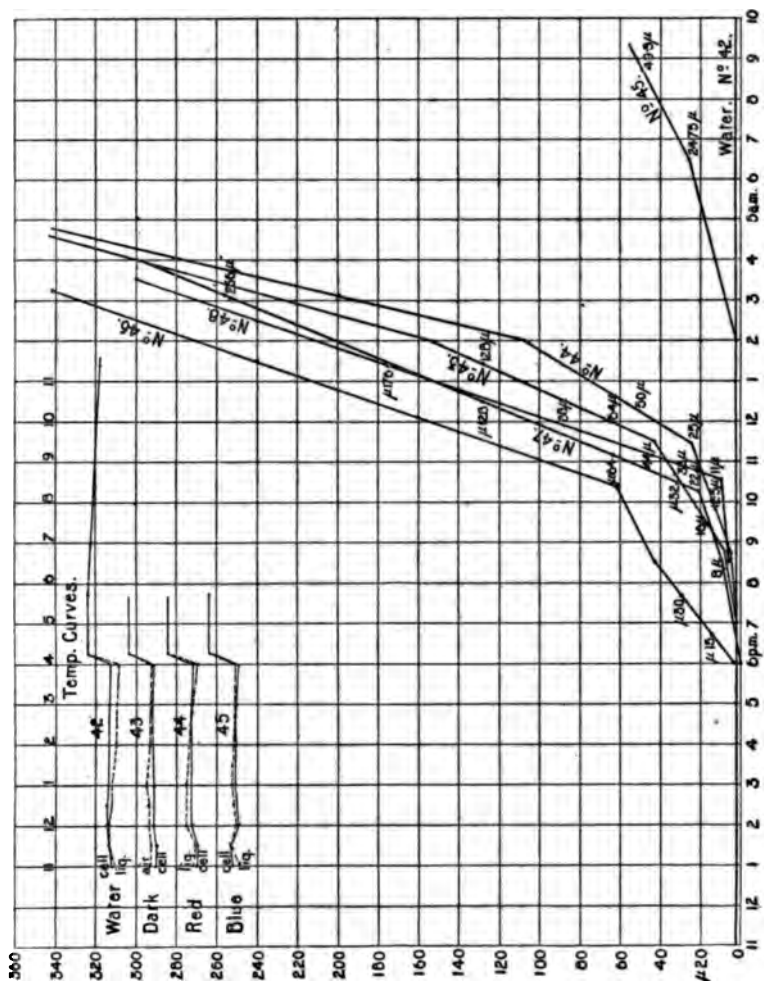
The day was a brilliantly hot and sunny one, but with light cumulus clouds floating slowly in the blue.

At 2 P.M. I selected a germinated rodlet, well out, under each microscope, and kept it under observation the whole period. This was made feasible by placing the screens below the stage of the microscope—the upper surface of the light cultures being shaded by

\* Brefeld found that such was the case when the spores of *B. subtilis* were heated to boiling ('Unters. ü. Schimmelpilze,' B. IV, 1881, p. 51).

| Time.     | Culture 1.          |                |                | Culture 2.         |                 |                | Culture D.   |              |              |
|-----------|---------------------|----------------|----------------|--------------------|-----------------|----------------|--------------|--------------|--------------|
|           | Average.            | Extreme.       | Mean.          | Average.           | Extreme.        | Mean.          | Average.     | Extreme.     | Mean.        |
| 6.15 P.M. | $\mu$ .<br>4.25 (4) | $\mu$ .<br>2-6 | $\mu$ .<br>4.0 | $\mu$ .<br>7.4 (5) | $\mu$ .<br>2-13 | $\mu$ .<br>7.6 | $\mu$ .<br>— | $\mu$ .<br>— | $\mu$ .<br>— |
| 8.30 "    | 8.3 (7)             | 3-16           | 8.5            | 43.0 (3)           | 25-60           | 42.5           | 5-6          | —            | 5-6          |
| 10.15 "   | 19.0 (6)            | 10-32          | 21.0           | 59.0 (8)           | 27-95           | 61.0           | —            | —            | —            |
| 10.30 "   | —                   | —              | —              | —                  | —               | —              | 9.0 (3)      | 6-12         | 9.0          |
| 3.30 A.M. | —                   | —              | —              | —                  | —               | —              | 279.0 (5)    | 200-400      | 300.0        |
| 4.0 "     | 308.0 (5)           | 100-500        | 300.0          | 364.0 (5)          | 100-670         | 385.0          | —            | —            | —            |

Curves 46-48.



cardboard—and they were sufficiently transparent to allow of direct vision and measurements of the growing filaments.

The following tables show the results, with the temperatures, &c.

Culture over Water-screen (+ trace of  $\text{CuSO}_4$ ).

| Time.     | Length. | Interval. | Growth. | Rate.   | Temperature.         |                      |
|-----------|---------|-----------|---------|---------|----------------------|----------------------|
|           |         |           |         |         | Air.                 | Cell.                |
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . | $^{\circ}\text{C}$ . |
| 2.25 P.M. | 8.8     | —         | —       | —       | 25.0                 | 26.0                 |
| 2.45 "    | 9.24    | 20        | 0.44    | 0.02    | 25.0                 | 28.0                 |
| 2.55 "    | 13.2    | 10        | 3.96    | 0.4     | 25.0                 | 29.0                 |
| 3.10 "    | 19.8    | 15        | 6.6     | 0.4     | 25.0                 | 29.0                 |
| 3.25 "    | 21.2    | 15        | 4.4     | 0.3     | 15.0                 | 21.5                 |
| 3.50 "    | 26.4    | 25        | 2.2     | 0.09    | 13.0                 | 18.0                 |
| 4.25 "    | 39.6    | 35        | 13.2    | 0.38    | 20.25                | 18.25                |
| 4.40 "    | 48.4    | 15        | 8.8     | 0.6     | 20.0                 | 19.0                 |
| 5.5 "     | 70.4    | 25        | 22.0    | 0.9     | 19.5                 | 19.0                 |
| 5.30 "    | 96.8    | 25        | 26.4    | 1.6     | 19.0                 | 19.0                 |
| 5.45 "    | 114.4   | 15        | 17.6    | 1.2     | 19.0                 | 18.5                 |
| 6.0 "     | 136.4   | 15        | 22.0    | 1.5     | 18.5                 | 18.25                |
| 6.20 "    | 171.6   | 20        | 35.2    | 1.7     | 18.25                | 18.0                 |

This rodlet therefore grew  $162.8 \mu$  in three hours fifty-five minutes, which at constant rate, would give  $0.69 \mu$  per minute—i.e., 7.8 per cent. of the original length.

The filament doubled its length in the following periods:—

1.  $8.8$ — $17.6 \mu$  in 32 minutes at  $26$ — $29^{\circ}\text{C}$ .
2.  $17.6$ — $35.2$  „ 42 „  $29$ — $18.25^{\circ}\text{C}$ .
3.  $35.2$ — $70.4$  „ 52 „  $18.25$ — $19^{\circ}\text{C}$ .
4.  $70.4$ — $141$  „ 58 „  $19$ — $18.25^{\circ}\text{C}$ .

## Culture over Screen of K. Bichromate.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temperature.         |                      |
|-----------|---------|-----------|---------|---------|----------------------|----------------------|
|           |         |           |         |         | Air.                 | Cell.                |
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . | $^{\circ}\text{C}$ . |
| 2.17 P.M. | 9.2     | —         | —       | —       | 24.0                 | 26.0                 |
| 2.35 "    | 14.46   | 18        | 5.26    | 0.3     | 24.0                 | 27.0                 |
| 2.55 "    | 27.6    | 20        | 13.14   | 0.65    | 24.0                 | 27.5                 |
| 3.10 "    | 41.4    | 15        | 13.8    | 0.9     | 24.0                 | 27.5                 |
| 3.25 "    | 55.2    | 15        | 13.8    | 0.9     | 14.5                 | 21.0                 |
| 3.50 "    | 73.6    | 25        | 18.4    | 0.74    | 12.0                 | 17.5                 |
| 4.25 "    | 92.0    | 35        | 18.4    | 0.52    | 20.5                 | 18.0                 |
| 4.40 "    | 112.7   | 15        | 10.7    | 0.7     | 20.5                 | 18.0                 |
| 5.5 "     | 144.6   | 25        | 31.9    | 1.27    | 19.5                 | 18.0                 |
| 5.30 "    | 179.4   | 25        | 34.8    | 1.4     | 19.0                 | 18.0                 |
| 5.45 "    | 216.2   | 15        | 36.8    | 2.4     | 18.6                 | 18.0                 |
| 6.0 "     | 246.1   | 15        | 29.9    | 2.0     | 18.5                 | 17.5                 |
| 6.10 "    | 257.6   | 10        | 11.5    | 1.15    | 18.5                 | 17.5                 |

The rodlet therefore grew  $248\frac{4}{5}\mu$  in three hours fifty-three minutes, which gives over  $1\mu$  per minute at constant rate—i.e., more than 10·8 per cent. of the original length.

The doubling periods =

1.  $9\cdot2-18\cdot5\mu$  in 25 minutes at  $26-27\cdot5^{\circ}\text{C}$ .
2.  $18\cdot5-37\cdot8$  „ 23 „  $27\cdot5-\text{constant}$
3.  $37-74$  „ 47 „  $27\cdot5-17\cdot5^{\circ}\text{C}$ .
4.  $74-148$  „ 75 „  $17\cdot5-18^{\circ}\text{C}$ .

**Culture in Dark.**

| Time.     | Length. | Interval. | Growth. | Rate.   | Temperature of air.  |
|-----------|---------|-----------|---------|---------|----------------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . |
| 2.30 P.M. | 6·6     | —         | —       | —       | 23·0                 |
| 2.45 „    | 11·0    | 15        | 4·4     | 0·3     | 23·5                 |
| 2.58 „    | 11·44   | 13        | 0·44    | 0·03    | 24·0                 |
| 3.10 „    | 13·2    | 12        | 1·76    | 0·15    | 24·0                 |
| 3.25 „    | 13·2    | 15        | 0·0     | 0·0     | 23·5                 |
| 3.50 „    | 15·4    | 25        | 2·2     | 0·09    | 23·0                 |
| 4.25 „    | 15·4    | 35        | 0·0     | 0·0     | 23·0                 |
| 4.40 „    | 17·6    | 15        | 2·2     | 0·12    | 22·0                 |
| 5.5 „     | 19·8    | 35        | 2·2     | 0·06    | 21·5                 |
| 5.30 „    | 22·44   | 25        | 2·64    | 0·1     | 21·0                 |
| 5.45 „    | 26·4    | 15        | 3·96    | 0·26    | 21·0                 |
| 6.0 „     | 28·6    | 15        | 2·2     | 0·12    | 20·75                |
| 6.30 „    | 35·2    | 30        | 6·6     | 0·22    | 20·5                 |
| 7.0 „     | 48·4    | 30        | 13·2    | 0·44    | 20·5                 |
| 8.0 „     | 79·2    | 60        | 30·8    | 0·5     | 20·0                 |
| 10.0 „    | 176·0   | 120       | 96·8    | 0·8     | 19·25                |

Doubling periods =

1.  $6\cdot6-13\cdot2\mu$  in 40 minutes at  $23-24^{\circ}\text{C}$ .
2.  $13\cdot2-26\cdot4$  „ 95 „  $24-21^{\circ}\text{C}$ .
3.  $26\cdot4-52\cdot8$  „ 86 „  $21-20^{\circ}\text{C}$ .
4.  $52\cdot8-105\cdot6$  „ 82 „  $20^{\circ}\text{C}$ .

I am unable to explain the slow rate of growth except on the hypothesis that the heat was rapidly radiated from the thin glasses on lifting the bell, and so kept the culture back.

Another possible—perhaps more probable—explanation is that the screens absorb (and subsequently radiate) heat from the solar rays; whereas the bell-jar did not do this to the same extent. The temperatures in the dark being, therefore, lower throughout all the early stages of the observation (only the air-temperature could be obtained owing to difficulties of manipulation) explains the effect.

Comparisons of the tables and curves of the two light cultures

bring out clearly the retarding action of the light, especially on noting that the temperature of the retarded culture was slightly higher, rather than lower, than the other.

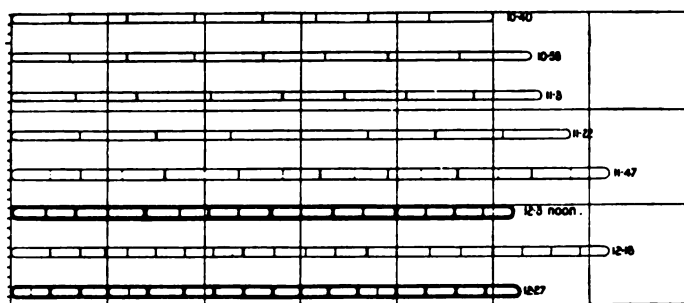
*Effects of Intense Insolation.*

Spores sown in a broth drop had germinated and developed good filaments in the dark at  $20-22^{\circ}\text{C}$ .; the culture was then put on a cool slab over-night, and kept slowly growing at  $10-12^{\circ}\text{C}$ .

At 10 A.M. the twenty-four hours culture had fine strong filaments when put at the south window under the  $1/12\text{th}$  immersion, with Zeiss' screw micrometer eye-piece, for exact measurements of the growths. Each division of the scale gave exactly  $2\mu$ .

The segment selected was an isolated one measuring just  $50\mu$  long, and consisting of four distinct segments, each again, but less obviously, divided by a median or nearly median septum into two cells.

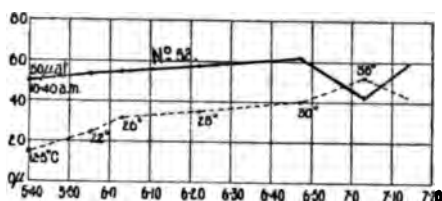
The diagrams below give the successive growths of these segments and cells, as observed from 10.40 to 12.27 under the power named, and fully bear out my previous observations that the growth is not perfectly uniform over the filament.



The following table summarises the total growth of the filament as a whole.

| Time.      | Length. | Interval.                   | Growth. | Rate.   | Temp. of cell.       |
|------------|---------|-----------------------------|---------|---------|----------------------|
|            | $\mu$ . | mins.                       | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . |
| 10.40 A.M. | 50      | —                           | —       | —       | 12.5                 |
| 10.55 "    | 54      | 15                          | 4       | 0.27    | 22.0                 |
| 11.3 "     | 55      | 8                           | 1       | 0.12    | 26.0                 |
| 11.22 "    | 58      | 19                          | 3       | 0.16    | 28.0                 |
| 11.47 "    | 62      | 25                          | 4       | 0.16    | 30.0                 |
| 12.3 P.M.  | 52      | Sharp contraction of whole. |         |         | 36.0                 |
| 12.18 "    | 62      | Had slowly regained length. |         |         | 26.0                 |
| 12.27 "    | 52      | Permanently contracted.     |         |         | 36.0                 |

Curve 52.



Here it must be noted that I was observing the filament in bright light, at relatively high temperatures; I attributed the slow growth from 10.55 to 11.47 to these factors.

The diagrams, which are drawn carefully to scale, give the relative elongations of the cells very accurately, and the reader is referred to them for details; the conclusion to be drawn from them is that from 10.40 to 10.55 the growth was entirely confined to the two right-hand segments, and then it occurred in the segments to the left end.

At 11.47 each of the right cells became divided, almost, if not quite, simultaneously, so that the filament now consisted of sixteen cells each from 3 to 4  $\mu$  long.

At 11.47 I was taking the light almost direct from the sun, and inadvertently allowed the cell to be more brilliantly illuminated than usual, and the reflected solar image in fact gradually got on to the culture-drop. On examining at 12.3 I found a most unexpected result—the whole filament had contracted from 62  $\mu$  (its length at 11.47) to 52  $\mu$ , and each of the 16 short cells composing it was now seen to be sharply contracted and lying distinct from its neighbour, from which it was separated by a broad cell wall. The whole was, in fact, now a series of cells inclosed in a distinct swollen sheath.

Still more remarkable was the fact that on again shading the culture, which at once brought the temperature down from 36 to 26° C., these cells at once closed up again, and the whole series once more slowly lengthened out, so that at 12.18 it was again 62  $\mu$  long. But no more growth occurred, though more septa were formed and the positions of others altered. On again turning the sun on, I saw these cells once more and *suddenly* contract away from one another, and clearly lie as contracted blocks of protoplasm in a distinct sheath.

Again they closed up, but more slowly, on shading the culture, and in ten minutes all the segments were marked with fine granular reticulations.

The cells were now dead, and we may conclude that the insolated protoplasm is killed by 5—10 minutes direct insolation at 35—36° C.

From subsequent experiments at these temperatures in the dark it may perhaps be concluded that the death here resulted from the high temperature, quite apart from any possible light action, and this

in spite of the fact that not all the cells were killed by the insolation (see below).

For my own part, I regard it as more probable that the high temperature merely *helped* the light action, for if the death was due to the high temperature, why did not all the cells die? That some escaped is intelligible if they were sheltered behind others—a by no means improbable event, for the culture was an advanced one, be it noted—whereas it is not easily explained on any assumption of heating or of poisoning.

It will be useful again to summarise the results of the foregoing section, and the suggestions they give rise to, before proceeding further.

1. So far as the spores exposed in the ripe resting condition are concerned, it may be regarded as proved that the blue-violet rays can retard or kill them, apart from any temperature effect, and it is shown that they are extremely resistant to high temperatures.

2. As regards actively growing bacilli, the evidence goes to show that the light action affects them also, by retarding their growth, and even eventually killing them, but, owing to certain difficulties, it is not so clearly shown quantitatively how far the effect is due to light alone, because the retarding effects are not observable *within* the short period during which measurements are taken.

3. In attempting to trace the effects of light (of moderate intensity only) it is so difficult to keep two growing filaments exactly at the same temperature under different conditions of illumination, that the quantitative results cannot be insisted upon too much in detail, for there is always the suspicion that even a difference of less than one degree of temperature may affect the rate of growth, and therefore alter the steepness or otherwise of the curve.

4. Some results suggest the possibility that the organism may even make use of rays at the red end of the spectrum in combatting the effect of those at the blue end.

5. The further possibility is suggested that the difference of behaviour between the spores and the filaments may simply depend on the relative activity of the protoplasm—that when the latter is comparatively inactive, as in the resting spore, it is incapable of resistance to the light action, but that, when it is actively engaged in metabolism and growth, it can resist the action if the temperature is favourable, and the more so the nearer the optimum the temperature is. On the other hand, extremes of temperature may favour the light-action.

6. Yet another hypothetical step may be taken. It is possible that the spores succumb so readily simply because a dangerously unstable supply of easily oxidisable materials is there, ready to be destroyed by the light-action; whereas, it is conceivable, these food-

materials are not present in the active cells of the growing filaments, for it is by no means improbable that the active protoplasm contains totally different compounds from the reserves in the inactive spore. Some such view is rather supported by the retarding action of the light on spore-formation.

7. It must not be forgotten that, in the majority of these experiments, the intensity of light employed is very low, and that the question investigated is not whether direct sunshine kills the organism (for there is no doubt on that head), but how far, and in what manner, ordinary daylight is effective.

*Experiments without Glass and at known Temperatures.*

As already mentioned, I contemplated avoiding glass altogether, in order to test the suspicion that some of the effects observed might be due to differences in diathermancy, &c.; this was done by employing thin quartz plates for all screens, floors of the cells, and even as cover-slips in some cases (though optical difficulties occurred here), and metallic surfaces for reflection.

The new apparatus took some time to get together, but I had some of the flat glass bottle-screens ground out by Mr. Hilger, and fitted with parallel plates of quartz, and used quartz floors to the cells, and even thin quartz cover-slips where necessary; then I replaced the mirror of each of the microscopes with one silvered in front, and was now in a position to repeat the foregoing observations with matters so arranged that none of the incident light could be absorbed by glass.

This is, perhaps, the best place to give the proofs that cultures standing side by side, and treated similarly in all respects, give growth curves so closely alike that we assume that the organism behaves similarly whenever subjected to the same conditions.

The following experiments show that the curves are alike in all essential respects, within certain limits of errors of measurement, when exposed to like conditions.

*Comparative Measurements under like Conditions.*

In order further to test the method of measurement, I made two cultures of spores sown in 10 per cent. gelatine and traces of broth and glucose, and kept at 22° C. in the dark incubator from 9 A.M. to 4 P.M. Germination had begun by 1 P.M., and at 4 P.M. the cultures were placed at 22° C. under microscopes, exactly alike, in diffuse light, the mirrors turned away in the intervals. A control thermometer cell was employed, and in each case a sturdy-looking filament selected of exactly the same length, as nearly as I could measure. Care was exercised to ensure that each scale measured evenly, and the measurements were made at various intervals, as follows:—

| Time.         | Lengths. |          | Temp.<br>° C. |
|---------------|----------|----------|---------------|
|               | A.<br>μ. | B.<br>μ. |               |
| 4.0 P.M. .... | 36.0     | 36.0     | 22.0          |
| 4.10 „ ....   | 42.75    | 43.83    | 22.0          |
| 4.32 „ ....   | 54.0     | 54.5     | 22.0          |
| 5.5 „ ....    | 76.5     | 77.0     | 22.0          |
| 5.35 „ ....   | 99.0     | 100.0    | 21.75         |
| 5.55 „ ....   | 119.25   | 121.5    | 21.75         |
| 6.5 „ ....    | 130.5    | 135.0    | 21.5          |
| 6.30 „ ....   | 157.5    | 166.5    | 21.5          |
| 6.45 „ ....   | 182.25   | 195.75   | 21.0          |
| 7.0 „ ....    | 204.75   | 222.75   | 21.0          |

It must be allowed, from the results, that the measurements attain a high degree of accuracy, but they suggest, at the same time, that the problem of detecting the exact effect of slight differences of illumination is extremely difficult for direct solution in this way.

After standing all night at 20° C., these two nearly equal cultures were exposed from 9 A.M. to 8 P.M. to a very blue sky, with passing white clouds, on July 4th, A over water, B over bichromate screen—quartz, silver mirror, shades, &c., as before—to see if spore-formation would be affected differently by the different lights. At 8 P.M., July 4, the final segmentations prior to spore-formation were visibly commencing in B, but neither showed spores as yet.

The following are the temperatures for the day, July 4:—

| Time.           | Temperatures. |            |
|-----------------|---------------|------------|
|                 | A.<br>° C.    | B.<br>° C. |
| 9.0 A.M. ....   | 17.5          | 17.5       |
| 10.0 „ ....     | 18.5          | 18.5       |
| 11.0 „ ....     | 20.25         | 20.0       |
| 11.45 „ ....    | 22.0          | 22.0       |
| 12.30 P.M. .... | 24.5          | 24.5       |

From 12.30 to 2.30 they were brought into the north laboratory, to avoid further rise of temperature; the temperature remained at 23°. At 2.30 they were returned to the south window.

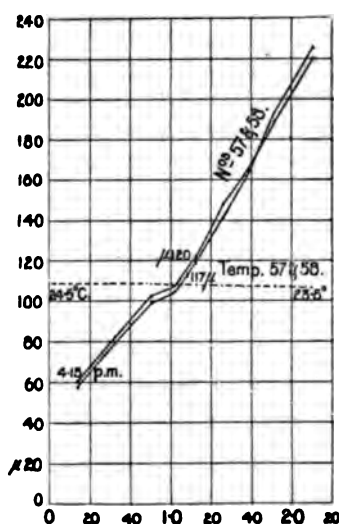
|                | ° C. | ° C. |
|----------------|------|------|
| 2.30 P.M. .... | 23.5 | 23.5 |
| 3.0 „ ....     | 22.0 | 22.0 |
| 7.0 „ ....     | 21.0 | 21.0 |
| 8.0 „ .. ....  | 21.0 | 21.0 |

Owing to partial drying up, I had to abandon the cultures, but it was clear that B was far in advance of A in respect of spore-forma-

tion. I fear that a new difficulty arises here—that of ensuring that each culture-drop shall contain the same amount of food-materials, if it is to last a long time; of course one does one's best to have the drops of equal size, and the drying up was here due to the different water contents of the cotton-wool in the arms of the cells.

In the following case the experiment was made in the north laboratory, fully exposed to the overcast sky, the day hot and sultry. The spores were sown in weak gelatine, as before, at 9 A.M., and were in the dark at 22° C. till 2 P.M. Germination was established at 2.30 and at 4.15; the measurements were begun on two filaments almost exactly the same length. The results testify strongly to the accuracy of the measurements and the equality of growth under like conditions (Curves 57 and 58).

| Time.          | Lengths. |          | Temp.<br>° C. |
|----------------|----------|----------|---------------|
|                | A.<br>μ. | B.<br>μ. |               |
| 4.15 P.M. .... | 58.5     | 60.0     | 24.5          |
| 4.21 „ .....   | 67.0     | 68.0     | 24.5          |
| 4.33 „ .....   | 80.0     | 82.0     | 24.5          |
| 4.50 „ .....   | 99.0     | 102.0    | 24.5          |
| 5.1 „ .....    | 105.75   | 108.0    | 24.25         |
| 5.12 „ .....   | 119.25   | 121.5    | 24.25         |
| 5.26 „ .....   | 141.75   | 148.5    | 24            |
| 5.37 „ .....   | 160      | 162      | 24            |
| 5.52 „ .....   | 193.5    | 189      | 23.75         |
| 6.10 „ .....   | 225      | 220      | 23.5          |



Note particularly the fall in the curve at 4.50 to 5.1, when the temperature began to fall; it shows how very sensitive to temperature-changes the *growing* filaments are, and how extremely difficult it is to eliminate this source of error.

Spores sown at 8.30 A.M., July 7, were kept at 22° C. in dark till 2.45 P.M., and then the selected rodlets treated exactly alike at the north window. Dull, cloudy, and eventually raining.

| Time.          | Lengths. |          | Temp.<br>° C. |
|----------------|----------|----------|---------------|
|                | A.<br>μ. | B.<br>μ. |               |
| 2.45 P.M. .... | 81.5     | 31.5     | 21.5          |
| 2.54 „ ....    | 36.0     | 35.0     | 21.5          |
| 3.36 „ ....    | 62.75    | 61.0     | 21.25         |
| 4.25 „ ....    | 99.0     | 96.75    | 20.75         |
| 4.48 „ ....    | 121.5    | 120.0    | 21.0          |
| 5.16 „ ....    | 160.5    | 154.0    | 20.0          |
| 5.43 „ ....    | 202.5    | 189.0    | 19.5          |

I now pass to the comparative cultures in light and dark, premising that each pair of cultures must be compared by itself, the difficulties of observation and preparation being so great that one cannot possibly push the method further.

As before, I begin with the exposures of ungerminated spores, and pass afterwards to exposures of already germinated rodlets and filaments in active growth.

On June 23 spores were sown in dilute broth-gelatine at 10 A.M., and exposed at once. The quartz cells, shades, and screens as before. The temperature at 10.30 was 19.5°, slowly falling at the open window to 19° C. at 12 noon, and exactly alike under both screens. The sky was overcast, with traces of hazy blue now and again, and a cool wind was blowing; no rain fell, but the afternoon was very dull. The following are the results, germination having begun soon after 12 noon.

| June 23.<br>Time. | Temperature. | Water.   |          |       |            |
|-------------------|--------------|----------|----------|-------|------------|
|                   |              | Average. | Extreme. | Mean. | Commonest. |
|                   | ° C.         | μ.       | μ.       | μ.    | μ.         |
| 2.0 P.M.          | 19.0         | 4.5—5.0  | —        | —     | —          |
| 4.30 „            | 17.25        | 6.0      | —        | —     | —          |
| 8.15 „            | 16.0         | 16.0 (8) | 9—22.5   | 15.75 | 15         |

| June 23.<br>Time. | Temperature. | K. Bichromate. |           |       |            |
|-------------------|--------------|----------------|-----------|-------|------------|
|                   |              | Average.       | Extreme.  | Mean. | Commonest. |
|                   | ° C.         | μ.             | μ.        | μ.    | μ.         |
| 2.0 P.M.          | 19·0         | 5              | —         | —     | —          |
| 4.30 "            | 17·5         | 9              | 6—12      | 9     | 8 or 9     |
| 8.15 "            | 16·0         | 47 (11)        | 13·5—76·5 | 45    | 50         |

The results are not so conclusive as in previous cases with brighter light, but, so far as they go, they bear out the inference already drawn, viz., that the light does inhibit the germination of the spores even though it is very feeble so far as blue rays are concerned.

The following experiment, carried out on June 30, a very hot day, with a slight haze but no clouds in the blue sky, bears out the foregoing conclusion that the action is due to intense destructive metabolism at high temperatures (see p. 375).

Spores were sown and exposed at 7 A.M., a more dilute gelatine, 5 per cent., with only traces of broth, being used. Control cultures in the dark at 24° C. showed that germination and growth can go on quite well in this medium, and filaments of 200 to 250 μ were developed by 3 P.M. from these controls put into the incubator at 7 A.M.

The temperatures reached were occasionally rather high, as the following table shows, but the exposed cultures were subjected to exactly the same influences, except as regards the light (never direct from the sun) filtered through the screens. The arrangement was exactly as before.

| Time.      | Temperatures.  |                     |
|------------|----------------|---------------------|
|            | Water.<br>° C. | Bichromate.<br>° C. |
| 7 A.M.     | 18·0           | 18·0                |
| 8.30 "     | 18·5           | 18·5                |
| 11 "       | 20·5           | 20·5                |
| 12 noon    | 25·0           | 25·0                |
| 12.30 P.M. | 27·0           | 27·0                |
| 3.30 "     | 31·0           | 31·0                |
| 9 "        | 19·0           | 19·0                |

At 3.30 P.M. the bichromate culture showed several germinating rodlets, and by 9 P.M. these had grown considerably. That the high temperatures and great range had retarded them, however, even in the orange light, was evident from the much better growth of the control cultures kept constant at 24° C. in the dark incubator. On July 1—i.e., next day—there were plenty of well-grown colonies, but

consisting of much broken filaments and rodlets, instead of forming long tresses.

The culture exposed over *water*, however, never gave any colonies at all: the spores were all killed.

That the light-action on the spores does not depend for its effect on a high temperature, however, follows not only from the previous experiments, but also, and very clearly, from the following one, carried out on July 1, a hot day with a cloudless blue sky, but somewhat hazy, probably owing to London smoke brought up the Thames valley by the light east wind then prevailing.

Spores were sown in 10 per cent. gelatine with traces only of broth and glucose, all the arrangements as before. The sowings were made and exposures started at the open south window at 9 A.M. The following table summarises the results:—

| Time.   | Temperature. |        | Remarks.                                                            |
|---------|--------------|--------|---------------------------------------------------------------------|
|         | Bichromate.  | Water. |                                                                     |
|         | ° C.         | ° C.   |                                                                     |
| 9 A.M.  | 19           | 19     |                                                                     |
| 10 "    | 20           | 20     |                                                                     |
| 10.30 " | 21           | 21     |                                                                     |
| 11.0 "  | 22           | 22     |                                                                     |
| 11.30 " | 24           | 24.5   | Brought into north laboratory.                                      |
| 12 noon | 24           | 24     |                                                                     |
| 1 P.M.  | 23.5         | 23.5   | Germination begun over bichromate.                                  |
| 2 "     | 23.5         | 23.5   |                                                                     |
| 3.30 "  | 24.0         | 24.0   | Germination over water beginning.<br>Returned both to south window. |
| 4 "     | 24.25        | 24.25  |                                                                     |
| 5 "     | 24           | 24     |                                                                     |
| 6 "     | 23.5         | 23.5   |                                                                     |
| 7.30 "  | 22.5         | 22.5   |                                                                     |

At 3.30 several of the filaments under bichromate were measured, and gave from 9 to 10  $\mu$  to 20  $\mu$ , and even greater lengths (one was nearly 40  $\mu$ ), whereas the most I could find in the culture over water were a few doubtful cases of commencing germination.

At 5 P.M. the shortest of the filaments over bichromate were about 30  $\mu$ , and the longest 130  $\mu$ . Twelve measurements gave 30, 36, 30, 45, 54, 45, 90, 99, 76.5, 135, 67.5, and 67  $\mu$  respectively, whereas no measurable filaments could be found in the culture over water, a few doubtful rods of 4 or 5  $\mu$  alone occurring.

Moreover, it was evident that the bichromate-culture filaments were growing very rapidly, and one was selected for observation, with the following results:—

| Time.  | Length. | Interval. | Growth. | Rate.   | Temp.         |
|--------|---------|-----------|---------|---------|---------------|
|        | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 5 P.M. | 112.5   | —         | —       | —       | 24.0          |
| 5.6 „  | 121.5   | 6         | 9       | 1.5     | 23.75         |
| 5.11 „ | 130.5   | 5         | 9       | 1.8     | 23.6          |
| 5.20 „ | 148.5   | 9         | 18      | 2.0     | 23.6          |
| 5.25 „ | 157.5   | 5         | 9       | 1.8     | 23.6          |
| 5.32 „ | 175.5   | 7         | 18      | 2.57    | 23.6          |
| 5.35 „ | 180.0   | 3         | 4.5     | 1.5     | 23.6          |
| 5.40 „ | 193.5   | 5         | 13.5    | 2.7     | 23.6          |
| 5.45 „ | 202.5   | 5         | 9.0     | 1.8     | 23.5          |
| 5.47 „ | 207.0   | 2         | 4.5     | 2.25    | 23.5          |
| 5.54 „ | 220.5   | 7         | 13.5    | 1.9     | 23.5          |
| 5.56 „ | 225.0   | 2         | 4.5     | 2.25    | 23.25         |

That is to say, this filament doubled its length in fifty-six minutes at 24—23.25° C.

Further measurements were made during the evening, and showed that the rapid growth continued, for at 6.40 P.M.—the temperature being 23° C.—this filament was approximately 450  $\mu$  long, and at 7.30— $t = 22.5^{\circ}$ —considerably over 1000  $\mu$ .

At the same time (7.30 P.M.) and at exactly the same temperature the culture over water showed one or two filaments, very feeble and sharply segmented, about 100  $\mu$  long.

At 10 P.M., temp. 22.5° in both cells, the long coils of the bichromate culture were several thousand  $\mu$  long, whereas the feebly growing one or two filaments of the water-culture measured from 150 to 250  $\mu$  at most, and were much broken up and starved looking.

Even on July 2, when examined at 8.30 A.M., the water-culture had no filament so long as 500  $\mu$ , though one or two were over 400  $\mu$  long, but by this time the filaments of the other culture measured many thousand  $\mu$ .

By 9 A.M. on July 3 the bichromate-culture was fully grown, and had developed magnificent spores in every filament; the water-culture showed no trace of spores, though the shortly segmented filaments were very granular.

These facts are of especial interest and importance as disposing effectually of any question as to so weak a food material being unsuitable for the growth, &c., of this bacillus. It is quite clear that very little is needed for normal development.

On July 4, after another day's exposure—but July 3 was dull, hot, and cloudy all day—the culture over water was still devoid of spores. After the whole of the 4th, in dark incubator at 22° C., still no signs of spores.

On July 5 one or two spores were detected at isolated spots here and there in the filaments, but very few.

It was perfectly clear that, exposed at the same temperatures, the culture from which the blue rays were screened flourished well and formed spores, whereas the other was starved and retarded, and could scarcely form spores at all.

Spores sown as before, in weak gelatine, at 8.30 A.M., July 7, were at once exposed to the south window. At 9.30, however, I removed them to the upper room, north window, to avoid high temperatures.

The day opened very bright, with a deep blue sky, about half covered with cumulus clouds, at noon became more overcast, and after 1 P.M. was dull and quite overcast; rain later.

#### Water.

| Time.     | Length. | Interval.        | Growth. | Rate. | Temp. |
|-----------|---------|------------------|---------|-------|-------|
|           |         |                  |         |       | ° C.  |
| 9.30 A.M. | —       | —                | —       | —     | 20    |
| 11.0 "    | —       | —                | —       | —     | 20.5  |
| 12 noon   | —       | —                | —       | —     | 22    |
| 1 P.M.    | —       | Germinating out. |         | —     | 22    |
| 2.15 "    | —       | —                | —       | —     | 21.75 |

At 3 P.M. I selected a good rodlet for measurement.

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp. |
|----------|---------|-----------|---------|---------|-------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 3.0 P.M. | 12.5    | —         | —       | —       | 20.5  |
| 3.30 "   | 15.75   | 30        | 2.25    | 0.07    | 21.0  |
| 3.50 "   | 18.0    | 20        | 2.25    | 0.11    | 20.5  |
| 4.17 "   | 27.0    | 27        | 9.0     | 0.33    | 20.5  |
| 6.0 "    | 76.5    | 103       | 49.5    | 0.48    | 19.0  |
| 6.30 "   | 90.0    | 30        | 23.0    | 0.76    | 19.0  |
| 7.40 "   | 135.0   | 80        | 45.0    | 0.56    | 18.0  |

Curve 63, p. 368.

Doubling periods:—

12.5—25  $\mu$  = 3 P.M.—4.12 P.M. = 72 minutes at 20.5—21—20.5° C.

25 — 50 " = 4.12 " — 5.5 " = 53 " 20.5—19.5° C.

50 — 100 " = 5.5 " — 6.46 " = 101 " 19.5—19° C.

Reckoned back:—\*

135 — 67.5  $\mu$  = 5.40 P.M.—7.40 P.M. = 120 minutes at 20—18° C.

67.5 — 33.75 " = 4.30 " — 5.40 " = 70 " 20.5—20° C.

33.75 — 16.75 " = 3.44 " — 4.30 " = 46 " 21—20.5° C.

\* I.e., the doubling periods calculated back on the curve from the last observation instead of forwards from the first one, and thus giving comparisons of the growth in different sections of the filament.

## Bichromate.

| Time.     | Length.     | Interval. | Growth. | Rate. | Temp.        |
|-----------|-------------|-----------|---------|-------|--------------|
| 9.30 A.M. | —           | —         | —       | —     | ° C.<br>20·0 |
| 11.0 "    | —           | —         | —       | —     | 20·5         |
| 12.0 noon | Germinating | out.      | —       | —     | 22·0         |
| 1.0 P.M.  | —           | —         | —       | —     | 22·0         |

I now selected a rodlet for measurement :—

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 2.15 P.M. | 24·75   | —         | —       | —       | 21·75 |
| 3.0 "     | 42·75   | 45        | 18·0    | 0·4     | 20·5  |
| 3.30 "    | 56·25   | 30        | 13·5    | 0·45    | 21·0  |
| 3.50 "    | 72·0    | 20        | 15·75   | 0·79    | 20·5  |
| 4.17 "    | 99·0    | 27        | 27·0    | 1·0     | 20·5  |
| 6.0 "     | 270·0   | 103       | 171·0   | 1·6     | 18·5  |
| 6.30 "    | 324·0   | 30        | 54·0    | 1·8     | 18·5  |
| 7.40 "    | 585·0   | 80        | 261·0   | 3·26    | 18·0  |

Curve 64, p. 368.

Doubling periods :—

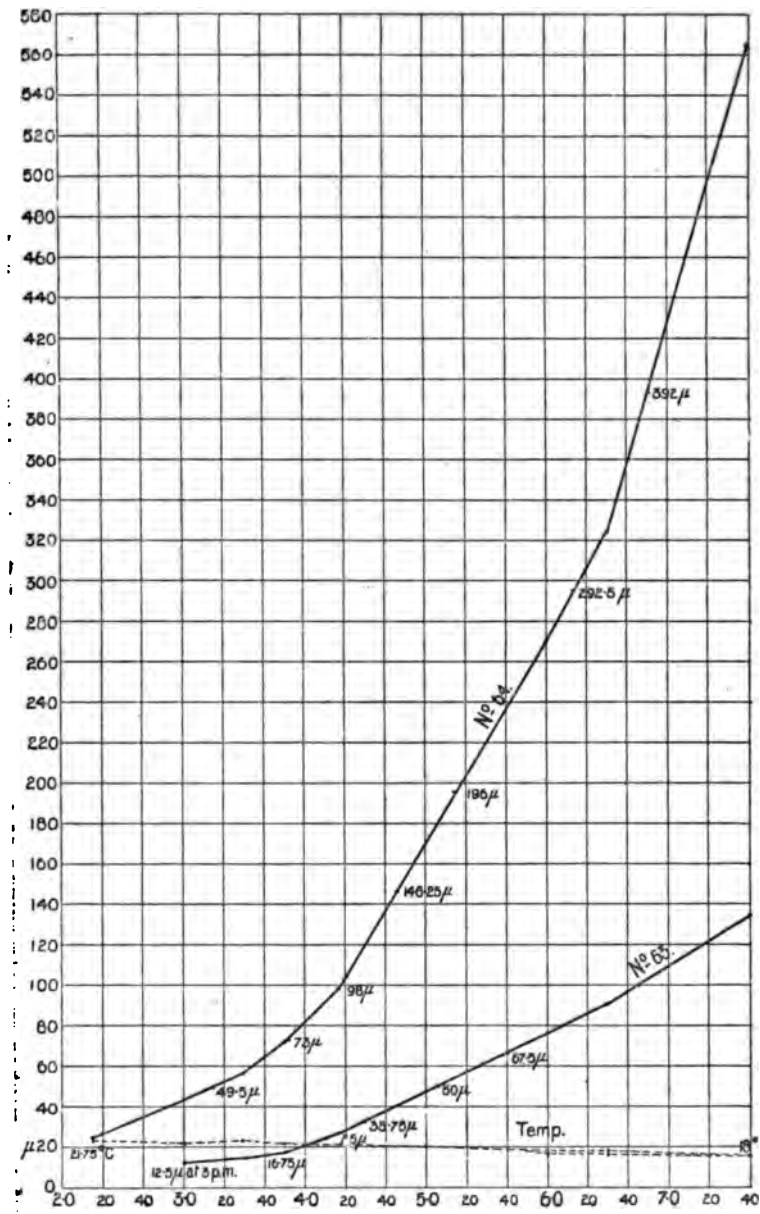
24·75—49·5  $\mu$  = 2.15 P.M.—3.15 P.M. = 60 minutes at 21·75—20·75° C.  
 49·5—98·0 " = 3.15 " —4.16 " = 61 " 20·75—20·5° C.  
 98·0—196·0 " = 4.16 " —5.15 " = 59 " 20·5—19·5° C.  
 196·0—392·0 " = 5.15 " —6.48 " = 93 " 19·5—18·5° C.

Reckoned back :—

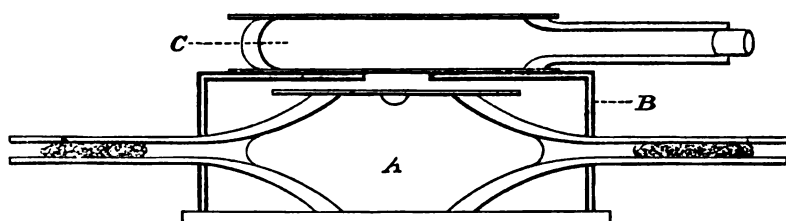
585·0—292·5  $\mu$  = 6.14 P.M.—7.40 P.M. = 86 minutes at 18·5—18·0° C.  
 292·5—146·25 " = 4.45 " —6.14 " = 89 " 20·0—18·5° C.  
 73·0—36·5 " = 3.51 " —4.45 " = 54 " 20·5—20·0° C.

Here, again, we see that the light effect on *spores* is obvious enough, and that with daylight, which gives no measurable results with *growing* filaments at all. We obtain a more vivid impression of the differences of behaviour of these two cultures if we note the following points :—

The water-culture took 101 minutes to grow from 50  $\mu$  long to 100  $\mu$ —double its length—at 19·5—19° C., and 120 minutes to grow from 67·5  $\mu$  to 135  $\mu$  at 20—18° C.; whereas the bichromate-culture grew from 292·5  $\mu$  to 585  $\mu$  (*i.e.*, doubled its length) in only 86 minutes at 18·5 to 18° C., and only took 89 minutes to grow from 146·25 to 292  $\mu$  at 20—18·5°.



On August 3 spores were sown in broth + 1 per cent. gelatine, at 10 a.m., and exposed as follows. Each of two cultures had a blackened matchbox cover pierced with a hole, so as to diminish radiation



A, culture cell (quartz floor, &c.) arranged as usual; B, an inverted matchbox painted black and with a hole over the hanging drop; C, a quartz bottle screen of water or bichromate, placed over the hole, so that no light can reach the hanging drop except through the screen.

and support the (quartz) screen. Each stood side by side, with a dummy cell provided with a thermometer, one pair (culture and dummy) under bichromate, the other under water.

They were on a table outside the north laboratory, exposed to the blue sky, but with no trace of direct sunlight. The amount of white cloud increased from 10.45 to 11.15, and very little open sky was available, except at intervals. The details concerning temperature and weather are given in the following table:—

| Time.      | Temperature. |        | Weather.                               |
|------------|--------------|--------|----------------------------------------|
|            | Bichromate.  | Water. |                                        |
|            | ° C.         | ° C.   |                                        |
| 10.10 A.M. | 17.5         | 17.5   | Blue sky, with scattered white clouds. |
| 10.15 "    | 17.75        | 17.75  |                                        |
| 10.34 "    | 17.9         | 17.9   |                                        |
| 10.55 "    | 18.75        | 18.75  | Clouding over badly.                   |
| 11.15 "    | 20.1         | 20.1   |                                        |
| 11.45 "    | 21.5         | 21.1   | Threatening rain.                      |
| 12.5 P.M.  | 21.7         | 21.4   | A few drops of rain.                   |
| 12.40 "    | 23.25        | 23.0   | Blue patches, but much cloud.          |
| 12.55 "    | 23.1         | 23.0   |                                        |
| 1.50 "     | 21.5         | 21.2   | Very dull.                             |
| 2.15 "     | 20.6         | 20.5   | Blue sky, few clouds.                  |
| 2.40 "     | 20.5         | 20.5   | Some blue, but many fleecy clouds.     |
| 3.20 "     | 19.3         | 19.3   | More blue.                             |
| 3.55 "     | 20.5         | 20.6   | Much cloud and very little blue.       |
| 4.40 "     | 19.0         | 19.1   |                                        |
| 5.0 "      | 17.8         | 17.9   | Blue sky, but hazy, and a few clouds.  |
| 5.38 "     | 17.5         | 17.5   |                                        |
| 6.5 "      | 16.6         | 16.5   | Blue, but hazy, and some cloud.        |
| 6.30 "     | 15.5         | 15.5   |                                        |
| 6.55 "     | 15.4         | 15.4   |                                        |

At 7 P.M. all were brought into the laboratory, and the temperature rose to 16.25° at 7.5, 18° at 7.20, 18° at 8.45.

The cultures were examined at 4.40, 7 P.M., and 8.45, in each case with no traces of germinated rodlets in that under water, whereas that under bichromate had germinated out normally and grown well, as the table shows.

| Time.                  | Bichromate.       |           |         |       | Water.                                |                 |         |       |
|------------------------|-------------------|-----------|---------|-------|---------------------------------------|-----------------|---------|-------|
|                        | Average.          | Extremes. | Mean.   | Temp. | Average.                              | Extremes.       | Mean.   | Temp. |
|                        | $\mu$ .           | $\mu$ .   | $\mu$ . | °C.   | $\mu$ .                               | $\mu$ .         | $\mu$ . | °C.   |
| 4.40 P.M.              | 70 (15)           | 31—139    | 85      | 19.0  | No trace                              | of germination. |         | 19.0  |
| 7.0 "                  | 225 (10)          | 156—270   | 216     | 16.75 | "                                     | "               | "       | 16.75 |
| 8.45 "                 | 330 (7)           | 270—460   | 365     | 18.0  | No rodlets, but a few spores swollen. |                 |         | 18.0  |
| 7.0 A.M. }<br>Aug. 4 } | Unmeasurably long |           | —       | 18.0  | No rodlets at all.                    |                 | —       | 18.0  |

How the conclusion that the light from a sky only occasionally blue, and generally very cloudy, retarded the spores indefinitely is to be avoided here seems inconceivable. There is nothing in the minute differences of temperature—the maximum at 11.45 was only 0.4 of a degree—to account for it, and as for the variations (from 17.5 to 23.25), even if they had attained dangerous ranges, they were similar in both cases; but they were in no way dangerous; the range was really very favourable.

On August 4 this experiment was repeated, but with broth instead of 1 per cent. gelatine drops. The cultures were started at 10.15, and kept till 10.55, at a north window in the laboratory, because rain was threatened. The sky then became blue in patches—a very deep blue, but with rolling heavy cumulus clouds—and the cultures were then taken outside and exposed exactly as yesterday, all the cells standing on a glass mirror, the surface of which was covered with black paper except just below the drops and the thermometer bulbs. The variations of sky and temperature are given in the annexed table.

| Time.          | Temperature. |        | Weather, &c.                                                      |
|----------------|--------------|--------|-------------------------------------------------------------------|
|                | Bichromate.  | Water. |                                                                   |
|                | ° C.         | ° C.   |                                                                   |
| 10.25 A.M.     | 19.5         | 19.6   | Very dull and threatens rain, therefore in laboratory.            |
| 10.57 "        | 20.4         | 20.3   |                                                                   |
| 11.15 "        | 18.1         | 18.4   |                                                                   |
| 11.47 "        | 23.0         | 23.6   | Blue sky and rolling cumulus.                                     |
| 12.30 P.M.     | 23.6         | 24.0   | Dull and cloudy.                                                  |
| 1.6 "          | 22.8         | 23.2   | Threatens rain.                                                   |
| 2.25 "         | 19.75        | 20.2   | Very thick clouds.                                                |
| 2.57 "         | —            | —      | Brought in to examine. Out at 3.10.                               |
| 3.10 "         | 19.1         | 19.75  | Dull, cloudy, threatens rain. Brought in to examine, and kept in. |
| 4.0 "          | 17.75        | 18.0   |                                                                   |
| 4.15 "         | 18.25        | 18.25  |                                                                   |
| 5.15 "         | 18.5         | 18.5   | In laboratory at north window.                                    |
| 7.0 "          | 18.0         | 18.0   |                                                                   |
| 8.20 "         | 18.0         | 18.0   |                                                                   |
| 8 A.M., Aug. 5 | 17.0         | 17.0   |                                                                   |

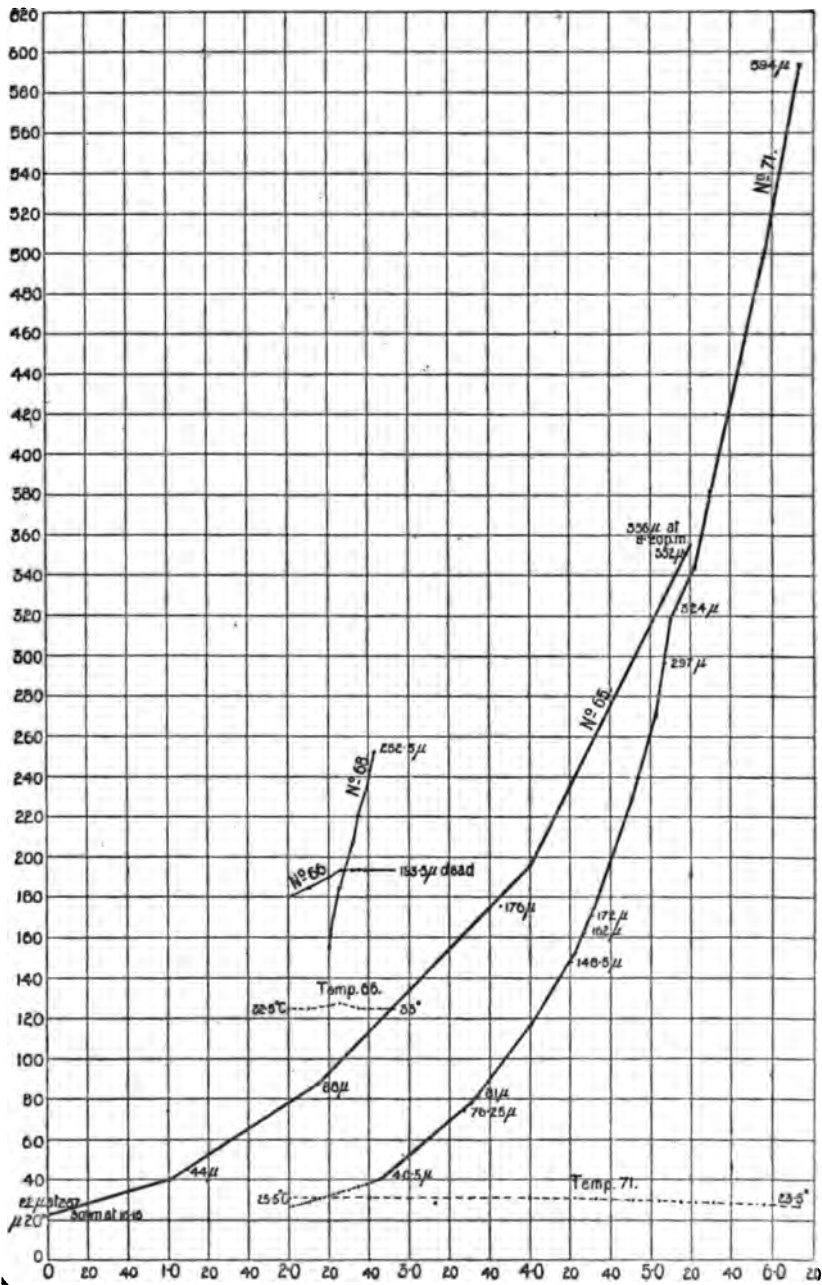
The results of examinations at 2.57 P.M., 4 P.M., &c., are given in the annexed table. They prove conclusively the evident light-action.

| Time.                  | Bichromate.                 |             |       | Water.                                                         |           |       |
|------------------------|-----------------------------|-------------|-------|----------------------------------------------------------------|-----------|-------|
|                        | Average.                    | Extremes.   | Mean. | Average.                                                       | Extremes. | Mean. |
|                        | μ.                          | μ.          | μ.    | μ.                                                             | μ.        | μ.    |
| 2.57 P.M.              | 22 (10)                     | 18.0—27.0   | 22.5  | No trace of germination.                                       |           |       |
| 4.0 "                  | 40 (13)                     | 18.0—67.5   | 42.75 | "                                                              | "         | "     |
| 5.15 "                 | 88 (10)                     | 36.0—184.0  | 110.0 | "                                                              | "         | "     |
| 7.0 "                  | 175 (10)                    | 40.0—290.0  | 165.0 | "                                                              | "         | "     |
| 8.20 "                 | 356 (10)                    | 265.0—450.0 | 357.5 | "                                                              | "         | "     |
| 8.0 A.M. }<br>Aug. 5 } | Immeasurably long tressses. |             |       | No signs of germination, though plenty of dead spores present. |           |       |

Curve 65, p. 372.

If we take the doubling periods of the bichromate-culture, we have (averages)

1. 22—44 μ = 2.57 P.M.—4.8 P.M., *i.e.*, 71 minutes at about 19.75—17.75° C.
2. 44—88,, = 4.8 " —5.15 " *i.e.*, 67 " " 17.75—18.5° C.
3. 88—176,, = 5.15 " —6.45 " *i.e.*, 90 " " 18.5—18.0° C.
4. 176—352,, = 6.45 " —8.19 " *i.e.*, 94 " " 18.0—17.0° C.



Of course, we must remember these numbers are for averages of lengths, and cannot be compared too closely with the numbers got as averages of detailed observations. Nevertheless, there is a certain and more than merely rough agreement, which suggests that the growth was normal for the rather wide ranges of temperature.

May 22 opened very clear and blue, but with a cold N.E. wind; after 10 A.M. it clouded over, and the afternoon was dull and wet as well as cold.

A culture of spores kept at 12° C. overnight was germinating freely, but slowly. The cell had a quartz floor; the drop was broth-gelatine.

At 10 A.M. this was put over a concave silver mirror and quartz cell of water, and the measurements begun. The exposure was to the open blue sky to the south; no direct sunshine at all was allowed, a cardboard screen being used.

The measurements were as follows, the temperatures being those of a similar cell over a plane silver and bichromate quartz cell; experiments showed this registered the same temperatures.

The experiment was simply a trial to satisfy myself that the apparatus worked satisfactorily. As the following table shows, the growth was very slow, but six hours' exposure to the dull light did not kill the filaments.

The following table gives results:—

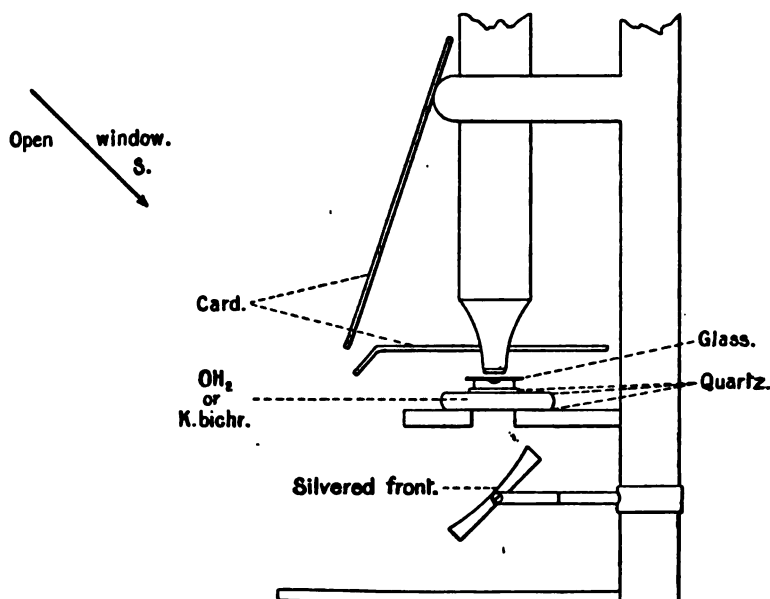
| Time.      | Length. | Interval. | Growth. | Rate. | Temp. |
|------------|---------|-----------|---------|-------|-------|
|            | μ.      | mins.     | μ.      | μ.    | ° C.  |
| 10.10 A.M. | 30      | —         | —       | —     | 12·0  |
| 11.50 „    | 38      | 100       | 6       | 0·06  | 13·0  |
| 12.30 P.M. | 44      | 40        | 8       | 0·2   | 13·0  |
| *2.15 „    | 48      | 105       | 4       | 0·004 | 12·0  |
| 4.20 „     | 64      | 125       | 16      | 0·12  | 12·75 |

This filament doubled its length in 5 hours and 28 minutes at 12—13—12° C.

On June 21, which turned out a brilliant sunny day, with blue sky, a little hazy, and very hot, the following experiments were made; the apparatus being quartz cells, and screens, and double cardboard shades to microscopes arranged as in the accompanying diagram (p. 110).

The spores, sown in broth-gelatine the night before at 12° C., had germinated evenly to rodlets by 9 A.M., and were exposed at the open south window at 9.45, over screens of water and of K. bichromate, the silver mirrors being turned to the blue sky to the south.

\* The window was closed from 2.15 P.M. onwards.



After allowing the light, from the blue sky only, to act on the cultures for about  $2\frac{1}{2}$  hours, the following measurements were made of two filaments exactly the same length, as near as I could select them:—

Bichromate.

| Time.      | Length. | Interval. | Growth. | Rate.   | Temp.         |
|------------|---------|-----------|---------|---------|---------------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 12.20 P.M. | 90.0    | —         | —       | —       | 26.0          |
| 12.45 „    | 108.0   | 25        | 18.0    | 0.76    | 26.0          |
| 12.55 „    | 112.5   | 10        | 4.5     | 0.45    | 27.0          |
| Water.     |         |           |         |         |               |
| 12.25 „    | 90.0    | —         | —       | —       | 24.5          |
| 12.45 „    | 99.0    | 20        | 9.0     | 0.44    | 24.0          |
| 12.55 „    | 108.0   | 10        | 9.0     | 0.9     | 25.0          |

Here was evidence that the culture over water was growing less rapidly than that over bichromate, but since the growth of the filaments in the bichromate culture was rapidly approaching the long lengths which I cannot measure, I determined to try the effect of more direct insolation to see if the higher temperature and intensity would bring more rapid action. Consequently I tried the results of

so arranging the mirrors that the solar image was reflected up on to the cultures and thermometers, again using the same cultures, but selecting yet another filament in each of lengths as nearly equal as possible.

The results were somewhat startling and puzzling, as the following tables and curves show:—

## Water.

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp.         |
|----------|---------|-----------|---------|---------|---------------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 2.0 P.M. | 180.0   | —         | —       | —       | 32.5          |
| 2.10 "   | 184.5   | 10        | 4.5     | 0.45    | 32.5          |
| 2.20 "   | 189.0   | 10        | 4.5     | 0.45    | 33.0          |
| 2.26 "   | 193.5   | 6         | 4.5     | 0.75    | 34.0          |
| 2.36 "   | 193.5   | 10        | —       | —       | 33.0          |
| 2.42 "   | 193.5   | 6         | —       | —       | 33.0          |

Curve 66, p. 372.

The growth stopped, and further examination showed the filament was dead.

## Bichromate.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp.         |
|-----------|---------|-----------|---------|---------|---------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 2.20 P.M. | 157.5   | —         | —       | —       | 33.5          |
| 2.22 "    | 175.5   | 2         | 18.0    | 9.0     | 33.5          |
| 2.25 "    | 184.5   | 3         | 9.0     | 3.0     | 33.0          |
| 2.32 "    | 207.0   | 7         | 22.5    | 3.2     | 33.0          |
| 2.35 "    | 221.0   | 3         | 13.5    | 4.5     | 33.5          |
| 2.39 "    | 234.5   | 4         | 13.5    | 3.4     | 34.0          |
| 2.42 "    | 252.5   | 3         | 18.0    | 6.0     | 34.0          |

Curve 68, p. 372.

Here we see clearly enough that at high temperatures ( $32-34^{\circ}$  C.) the bright light acted as a powerful *stimulus to rapid growth* behind the bichromate, which warded off the blue-violet rays, but brought about rapid *diminution of growth and death* behind the water.

It seems extremely probable therefore that, provided there is plenty of food-material of a highly nutritious nature present, the high temperature is in itself merely conducive to intense constructive metabolism and growth, but if the blue rays gain access at the same time they so interfere with constructive metabolism or so promote *destructive* metabolism (perhaps by promoting respiration?) that rapid death ensues.

In any case the death cannot here be referred merely to the high temperature, because, on the whole, the bichromate culture was at a higher temperature than the other.

On July 5 spores were sown in weak gelatine at 9 A.M., and cultures kept in dark till 2 P.M. The sky was particularly blue, but unfortunately I had to abandon the measurements at 4.25 P.M., and on my return several hours later they had grown too long to measure.

So far as the measurements show there was no appreciable light-effect up to 4 P.M., but after that the bichromate curve was making a sharp rise out of all proportion to the temperatures; it was unfortunate that an engagement took me away from the observations just at the critical part, because the pursuit of this rise—which ought to have been less than the rise in the other curve, according to the temperatures—would have been interesting.

But these cultures were placed next morning at 22° C. in the dark incubator, and on the 7th July the one over bichromate had formed excellent and normal chains of spores; whereas the one over water, though some spores were developed, was far behind in that respect. So that, after all, there was an appreciable light effect in the retardation of spore formation.

Spores were sown at 12 midnight on July 5, and kept at 22° in dark incubator till 10 A.M. They were then exposed over a water screen, quartz, as usual. The weak gelatine was used. The day was very hot, and the haze in the early morning soon became thicker, and by noon the sky was overcast; the afternoon was dull and intensely sultry, ending in thunderstorms. From previous experience no light-effect could be expected, and the following table and curve show that no appreciable effect was obtained. The curve, on the other hand, may be regarded as an almost perfect type of the growth record at the temperature used, probably the optimum or a little beyond.

The measurements could only be conducted directly on the whole filament up to 5.3 P.M., but fortunately the filament then segmented so distinctly into two parts, and at 5.30 into three parts, that, since these remained in contact and each quite straight but forming very open angles one with another, I was able to measure each by itself and add their growths. After 6.14, however, this became no longer possible, partly owing to the enormously rapid growth and partly to the lengths being greater than I could measure accurately.

| Time.      | Length.        | Interval. | Growth. | Rate.   | Temp.         |
|------------|----------------|-----------|---------|---------|---------------|
|            | $\mu$ .        | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 10.0 A.M.  | —              | —         | —       | —       | 22.0          |
| 10.45 "    | } Germinating. | —         | —       | —       | 23.5          |
| 11.30 "    |                | —         | —       | —       | 26.0          |
| 12.0 noon  |                | —         | —       | —       | 25.5          |
| 12.30 P.M. |                | —         | —       | —       | 26.5          |
| 12.45 "    | 4.0            | —         | —       | —       | 26.5          |
| 2.0 "      | —              | —         | —       | —       | 25.5          |
| 2.45 "     | 40.5           | 135       | 36.5    | 0.27    | 26.0          |
| 3.30 "     | 78.75          | 45        | 38.25   | 0.85    | 26.0          |
| 4.0 "      | 117.0          | 30        | 38.25   | 1.27    | 26.0          |
| 4.22 "     | 153.0          | 22        | 36.0    | 1.6     | 25.5          |
| 4.32 "     | 175.5          | 10        | 22.5    | 2.25    | 25.5          |
| 4.51 "     | 229.5          | 19        | 54.4    | 3.0     | 25.0          |
| 5.3 "      | 265.5          | 12        | 36.0    | 3.0     | 25.0          |
| 5.15 "     | 319.5          | 12        | 54.0    | 4.5     | 24.75         |
| 5.21 "     | 342.0          | 6         | 22.5    | 3.7     | 24.5          |
| 5.30 "     | 382.5          | 9         | 40.5    | 4.5     | 24.5          |
| 5.40 "     | 436.5          | 10        | 54.0    | 5.4     | 24.0          |
| 5.56 "     | 499.5          | 16        | 63.0    | 4.0     | 24.0          |
| 6.14 "     | 594.0          | 18        | 94.5    | 5.25    | 23.5          |

Curve 71, p. 372.

Doubling periods:—

40.5—81.0  $\mu$  = 2.45 P.M.—3.33 P.M. = 48 minutes at 26° C.  
 81.0—162.0 „ = 3.33 „ —4.27 „ = 54 „ 26—25.5° C.  
 162.0—324.0 „ = 4.27 „ —5.13 „ = 46 „ 25.5—24.75° C.

Reckoned back:—

594.0—297.0  $\mu$  = 5.7 P.M.—6.14 P.M. = 67 minutes at 25.0—23.5° C.  
 297.0—148.5 „ = 4.20 „ —5.7 „ = 47 „ 25.5—25.0° C.  
 148.5—76.25 „ = 3.27 „ —4.20 „ = 53 „ 26.0—25.5° C.

On August 6 spores sown in two broth-drop cultures were kept at 23° from 10.15 A.M. to 2 P.M., and germinated out well.

Then placed over bichromate and water respectively during the afternoon to test the light-action, with the following results (pp. 378, 379), a rodlet 6.75  $\mu$  long being fixed in the water culture, and one 13.5  $\mu$  long in the bichromate culture.

The morning had been very dull, but cleared up between 1 and 2 P.M.; at 2.30 the sky was only just showing blue and hazy between streaky clouds, little or no wind. From 2.37 to 3.10 the sky was overcast with heavy dull clouds, but cleared a bit by 3.30, and from 3.50 to 4.45 again showed some hazy blue between streaky clouds. The clouds get thicker, in fleecy streaks afterwards, and there was some blue showing up to 4.50. Thence onwards was dull and overcast.

The window was open—south laboratory—all the time, up to 6.55, quartz, silver mirrors, card and matchbox screens, &c., as before.

The light from the blue sky was brilliant for ten minutes or a quarter of an hour at the beginning of the exposure.

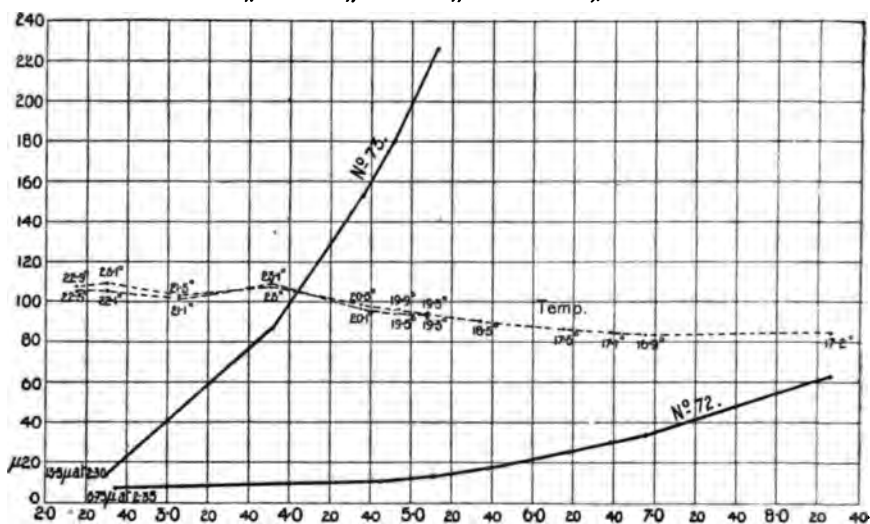
## Water.

| Time.          | Length. | Interval. | Growth. | Rate.   | Temp.         |
|----------------|---------|-----------|---------|---------|---------------|
|                | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 2.15 P.M.      | —       | —         | —       | —       | 22.5          |
| 2.35 "         | 6.75    | —         | —       | —       | 22.1          |
| 2.37 "         | —       | —         | —       | —       | 22.1          |
| 2.52 "         | 6.75    | 17        | —       | —       | 22.6          |
| 3.4 "          | —       | —         | —       | —       | 21.1          |
| 3.51 "         | 6.75    | 59        | —       | —       | 23.1          |
| 4.35 "         | 11.25   | 44        | 4.5     | 0.1     | 20.1          |
| 4.52 "         | 13.5    | 17        | 2.25    | 0.1     | 19.5          |
| 5.11 "         | 13.5    | 19        | —       | —       | 19.3          |
| 5.39 "         | 18      | 28        | 4.5     | 0.16    | 18.5          |
| 6.17 "         | 24.75   | 38        | 6.75    | 0.17    | 17.5          |
| 6.28 "         | 27      | 11        | 2.25    | 0.2     | 17.3          |
| 6.39 "         | 29.25   | 11        | 2.25    | 0.2     | 17.1          |
| Window closed. | 6.55 "  | 16        | 4.5     | 0.3     | 16.9          |
|                | 8.25 "  | 90        | 29.25   | 0.32    | 17.2          |

Curve 72.

Doubling periods :—

1.  $6.75 - 13.5 \mu = 2.35 \text{ P.M.} - 4.52 \text{ P.M.} = 137 \text{ minutes at } 22.1 - 19.5^{\circ} \text{ C.}$
2.  $13.5 - 27 \text{ " } = 4.52 \text{ " } - 6.28 \text{ " } = 96 \text{ " } 19.5 - 17.3^{\circ} \text{ C.}$
3.  $27.0 - 54 \text{ " } = 6.28 \text{ " } - 7.57 \text{ " } = 89 \text{ " } 17.3 - 16.9^{\circ} \text{ C.}$



Bichromate.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp.         |
|-----------|---------|-----------|---------|---------|---------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 2.15 P.M. | —       | —         | —       | —       | 22.9          |
| 2.30 "    | 13.5    | —         | —       | —       | 23.1          |
| 2.37 "    | —       | —         | —       | —       | 22.5          |
| 3.4 "     | 42.75   | 11        | 2.25    | 0.2     | 21.5          |
| 3.52 "    | 85.5    | 48        | 42.75   | 0.89    | 23.0          |
| 4.36 "    | 153.0   | 44        | 67.5    | 1.5     | 20.5          |
| 4.53 "    | 180.0   | 17        | 27.0    | 1.6     | 19.9          |
| 5.12 "    | 225.0   | 19        | 45.0    | 2.4     | 19.3          |

Curve 73, p. 378.

Doubling periods :—

1. 13.5—27  $\mu$  = 2.30 P.M.—2.42 P.M. = 12 minutes at 22.9—23.1—22.5 $^{\circ}$  C.
2. 27.0—54 " = 2.42 " —3.17 " = 35 " 22.5—21.5 $^{\circ}$  C.
3. 54.0—108 " = 3.17 " —4.7 " = 50 " 21.5—23.0—21.0 $^{\circ}$  C.
4. 108.0—216 " = 4.7 " —5.9 " = 62 " 21.0—19.3 $^{\circ}$  C.

It is pretty evident, in spite of the irregular and almost abnormally rapid growth recorded for the bichromate culture at first, that a prolonged retardation occurred in the water culture—a retardation which, for broth cultures, is too long for the temperatures, and can only be referred to the light.

On November 16 I sowed spores in normal 10 per cent. gelatine, and put into 22 $^{\circ}$  C. at 10 A.M. Two cultures were made, and at 12.30 these were exposed as follows, to test the effect of the light from a blue sky on germinating rodlets. The two cultures stood side by side on a mirror, each covered with a matchbox screen; on one lay a blue glass, and on the other a red one. The sky was fairly clear, but clouds passed occasionally.

Two control-cells with thermometers lay beside them, and each of these was arranged exactly like its culture, the glasses used for screens being in each case the half of the same piece of glass as that on the culture.

The temperature was rather low, and ran as follows during the exposure from 12.30 to 4 P.M., on west side of laboratory, and completely sheltered from any direct sunlight.

|                | Temperatures.     |                  |
|----------------|-------------------|------------------|
|                | Blue cell.<br>°C. | Red cell.<br>°C. |
| 12.40 P.M..... | 12.75             | 12.0             |
| 12.50 „ .....  | 10.8              | 10.75            |
| 1.0 „ .....    | 10.4              | 10.3             |
| 2.0 „ .....    | 9.9               | 9.9              |
| 2.15 „ .....   | 9.5               | 9.5              |
| 3.0 „ .....    | 8.5               | 8.5              |
| 4.0 „ .....    | 7.0               | 7.0              |

At 4 P.M. the cultures were brought into the laboratory, where the temperature rose to 12.5° C., and there is no question as to the temperature being identical in both cells from 2 P.M. onwards, and at no time after the first ten minutes of exposure did the temperature differ by more than  $\frac{1}{10}$ th of a degree C.

When the cultures were first put out the spores were swollen, and were already beginning to germinate, and if they had remained at 22° C. till 4 P.M. they would have developed filaments from 90 to 150  $\mu$  long, as shown by two sister cultures left at 22° C. as controls, and which were examined at 4 P.M.

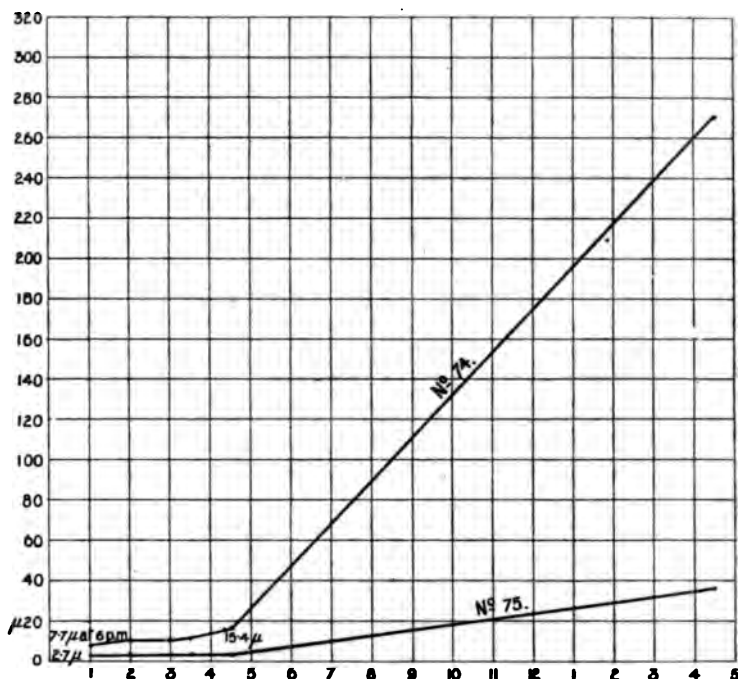
As it was, the exposure to a low temperature—12° to 7° C.—of course checked both cultures.

Nevertheless, at 6 P.M. the red culture showed several rodlets 9 to 10  $\mu$  long, one 6  $\mu$ , and two or three 4.5 to 5  $\mu$ , whereas I could discover plenty of swollen and germinated spores 2—3  $\mu$  long, and even one rodlet 4.5  $\mu$  long in the blue, but nothing longer. The following table gives the successive measurements.

It will be seen that here we have obvious retardation by  $3\frac{1}{2}$  hours of diffuse blue light acting on the young rodlets, and that this retardation was entirely independent of the temperature, which was the same in both cases. (See Curves 74 and 75.)

| Time.     | Temp. | Blue.     |           |       | Red.      |             |        |
|-----------|-------|-----------|-----------|-------|-----------|-------------|--------|
|           |       | Average.  | Extremes. | Mean. | Average.  | Extremes.   | Mean.  |
|           | ° C.  |           |           |       |           |             |        |
| 6.0 P.M.  | 12.5  | 2.7 (10)  | 4.5—2.5   | 3.5   | 7.7 (10)  | 4.5—10.0    | 7.25   |
| 7.0 „     | 13.0  | 2.75 (10) | 5.0—2.5   | 3.7   | 10.0 (7)  | 9.0—12.0    | 10.5   |
| 8.30 „    | 12.75 | 2.75 (10) | 5.0—2.5   | 3.7   | 11.5 (8)  | 9.0—14.0    | 11.5   |
| 9.30 „    | 12.8  | 2.75 (10) | 5.0—2.5   | 3.7   | 16.0 (7)  | 11.0—18.0   | 14.5   |
| 9.30 A.M. | 12.5  | 36.0 (6)  | 18.0—81.0 | 49.5* | 270.0 (9) | 157.5—360.0 | 258.75 |

\* The mean is raised by a single filament which was (81  $\mu$ ), much longer than any others.



The curves show that the doubling periods ran approximately thus:—

7.7—15.4 = 6 P.M. to 9.10 P.M. = 190 min. at 12.5—13—12.75 for the red, whereas the blue did not double (2.7—5.4 = 6 P.M. to 10.30 = 270 min. at same temperature) till 80 mins. later.

On November 17 spores which had been sown overnight in normal 10 per cent. gelatine at 13° C. had germinated to filaments 200 to 300 μ long by 10 A.M.; two cultures were examined and exposed at 10.15 A.M., at the south of the laboratory, care being taken that the sun was behind a building, and could not touch the cultures.

One culture was under blue and the other under red glass, each with its control thermometer cell, and side by side on a mirror, all open to the blue sky. The day was cold, and growth very slow. Matchbox screens were used.

The temperature ran as follows:—

| Time.      | Blue.<br>° C. | Red.<br>° C. |
|------------|---------------|--------------|
| 10.15 A.M. | 12.5          | 12.25        |
| 10.35 "    | 11.75         | 11.0         |
| 12.15 P.M. | 12.25         | 11.75        |
| 1.0 "      | 12.25         | 11.75        |
| 3.15 "     | 10.2          | 10.0         |

At 3.15—*i.e.*, after five hours' exposure—the red culture had grown considerably more than the blue one, although, so far as there was any difference of temperature at all, it favoured the blue culture. The filaments were too long, and too much curved to measure, but there was no doubt as to the increased growth.

Spores sown in normal gelatine at 10 P.M. on November 17, and kept in the dark at 13° C., had germinated to rodlets averaging 9–12  $\mu$  long at 8.30 A.M. on November 18, when two cultures were exposed to the clear blue sky on a mirror to the north and out of the sun. The day was cold and very fine. Each culture was covered by a black match-box screen, and had a thermometer control by its side treated exactly like it. One culture had ruby glass over it, the other blue; the latter was slightly in advance, *i.e.*, had some slightly longer rodlets. In order to be on the safe side I chose the culture which had a slight advance if anything for the blue.

The following table (p. 383) summarises the results of measurements, and the temperatures.

It is not difficult to translate these records. At the very low temperature used growth was extremely slow\* in both cases; nevertheless the temperature was above the minimum, and the red glass screen enabled it to go on as usual, because the inhibiting blue-violet rays were cut off, whereas behind the blue glass these rays carried on their destructive work, and the rodlets did not grow at all after the first hour or two.

A word of explanation is necessary, perhaps, regarding the slight discrepancies between the measurements in the blue.

Each drop had about 20 spores in it. In taking the successive observations, two sources of (minute) errors exist: one is, that no doubt the measurements, even of the same rodlet, are not perfectly accurate—it is easy to see how one could go wrong to 0.25, or even 0.5 of a  $\mu$ —the other is that one does not always find the *same* six or eight rodlets, however carefully one tries. My practice is to seek for the same, but to take care that *every* drop is thoroughly overhauled for the biggest and the smallest rodlets.

On the whole I think it will be admitted that the measurements correspond very well, and that the averages are very good.

At 9.15 P.M. the temperature was falling for the night, and it went down to below 9° C. before morning, rising again slowly after sunrise; this explains the still slow growth of the red culture.

At 8.30 A.M. I put both cultures into the incubator at 22° C., to see if a rise of temperature would make any difference as regards the dormancy of the blue culture.

The result was that while the red culture had filaments from 40 to 300  $\mu$  long by 12.30 noon, the blue one showed no further signs of

\* The doubling period would seem to be 360–400 minutes, or even longer.

| Time.      | Blue. |           |            |       | Red.  |           |           |       |
|------------|-------|-----------|------------|-------|-------|-----------|-----------|-------|
|            | Temp. | Average.  | Extremes.  | Mean. | Temp. | Average.  | Extremes. | Mean. |
| 8.30 A.M.  | ° C.  | μ.        | μ.         | μ.    | ° C.  | μ.        | μ.        | μ.    |
| 9.0 "      | 9.9   | 10-12     | —          | —     | 9.6   | 9-11      | —         | —     |
| 9.40 "     | 9.7   | —         | —          | —     | 9.1   | —         | —         | —     |
| 10.15 "    | 9.1   | —         | —          | —     | 9.0   | —         | —         | —     |
| 11.30 "    | 10.0  | —         | —          | —     | 10.25 | —         | —         | —     |
| 12.30 P.M. | 9.5   | —         | —          | —     | 9.5   | —         | —         | —     |
| 2.0 "      | 8.5   | 12.5 (6)  | 11.25-18   | 14.6  | 8.5   | 10.5 (6)  | 6.25-13.5 | 10    |
| 3.0 "      | 8.6   | —         | —          | —     | 8.5   | —         | —         | —     |
| 4.0 "      | 8.5   | —         | —          | —     | 8.6   | —         | —         | —     |
| 5.0 "      | 8.5   | 12.25 (6) | No growth. | —     | 8.5   | 20.25 (6) | 18-24.25  | 21    |
| 6.15 "     | 14.5  | —         | —          | —     | 14.5  | —         | —         | —     |
| 7.15 "     | 14.5  | 14.6 (6)  | 9-18       | 13.5  | 14.5  | 23.75 (5) | 18-36     | 27    |
| 8.15 "     | 14.5  | —         | —          | —     | 14.5  | —         | —         | —     |
| 9.15 "     | 14.0  | 13.5 (7)  | 9-20.25    | 14.6  | 14.0  | 31.5 (8)  | 18-45     | 31.5  |
| 8.30 A.M.  | 13.7  | —         | —          | —     | 13.6  | —         | —         | —     |
|            | 10.0  | 14.6 (8)  | 9-22.5     | 15.75 | 10.0  | 48.6 (8)  | 22.5-76.5 | 49.5  |

life than before. Similarly, at 4 P.M., the blue was as before, while the red had filaments several thousands of  $\mu$ 's in length.

On November 18 spores were sown in normal gelatine at 8 A.M., and kept at 22° C. till 12.30, when they had germinated to rodlets 8—9  $\mu$  long. Two cultures were then exposed at the south window, the sun being off (behind a building) on microscopes, and over blue and red glass respectively, the light reflected up from ordinary mirrors. Controls as usual. The whole day was clear and cold.

The following are the results:—

The doubling period for the red culture comes out (for the mean) =

$$24.75 - 49.5 \mu = 1.30 \text{ P.M.} - 3.47 \text{ P.M.} = 137 \text{ minutes at } 15.5 - 16.4 - 15^\circ.$$

Owing to the fact of my having the previously described cultures, exposed to the open sky behind the same blue and red screens, but at a lower temperature, it is not difficult to see what happened here. The amount of light reflected from the small glass mirrors of the microscopes is not sufficiently intense to kill the rodlets at these temperatures, though it is sufficient to inhibit their growth perceptibly.

This explains many of my previous failures. If I arrange the microscopes so as to throw a more intense daylight on the cultures, then the heat rays produce difficulties, because the screens transmit them in different proportions, and I either find the measured filament growing longer than can be measured before the inhibition sets in, or the difference in temperature between the two cultures so great that doubts arise as to how much of the inhibition is due to lower or higher temperature, and how much to the light action.

For, as we now see, the temperature is effective at once, but the light action takes a considerable time to make its effects visible on the growth-curve, and over and over again I have found cultures just beginning to show the retarding, brake-like action of the light injury at, or even after, the conclusion of the short growing period I am able to quantitatively examine and record. Of course, though one may be convinced by inspection that, of two cultures, one has formed a smaller crop than the other, in the absence of measurements the statement wants the definiteness I have been trying to attain.

On December 6, spores in normal gelatine were sown, and exposed at once at 11 A.M. to the hazy, winter sun, at south window of my house.

The "red" stood over bichromate; the "blue" over water tinged with  $\text{CuSO}_4$ , both in quartz cells.

The temperature of controls ran as follows during exposure:—

| Time.          | Red.<br>° C. | Blue.<br>° C. |                |
|----------------|--------------|---------------|----------------|
| 11.0 A.M. .... | 13.0         | 13.0          | Window<br>open |
| 11.30 " ....   | 11.75        | 12.5          |                |
| 12.30 " ....   | 11.5         | 12.25         |                |
| 12.40 " ....   | 12.25        | 13.0          |                |
| 1.0 P.M. ....  | 13.0         | 13.5          | Window<br>shut |
| 1.10 " ....    | 15.5         | 15.0          |                |
| 1.20 " ....    | 16.5         | 15.5          |                |
| 1.50 " ....    | 14.5         | 14.5          |                |
| 2.0 " ....     | 14.0         | 14.0          |                |
| 3.10 " ....    | 11.0         | 11.0          |                |

At 3.10 the exposure was stopped, and both cultures put into an incubator at 22° C. in the dark, and they remained under absolutely like conditions to the end of the experiment.

The marked retardation of the blue culture, after the four hours' exposure to a winter sun, never clear of haze, and at temperatures a trifle higher than the red rather than below, is well seen from the following table, in spite of the careful nursing at 22° C., a very favourable temperature, be it noted:—

| Time.     | Red.                                   |               |         | Blue.                 |           |         |
|-----------|----------------------------------------|---------------|---------|-----------------------|-----------|---------|
|           | Average.                               | Extremes.     | Mean.   | Average.              | Extremes. | Mean.   |
| 4.30 P.M. | μ.<br>4.5 (10)                         | μ.<br>3.0—5.0 | μ.<br>4 | μ.<br>No germination. | μ.<br>"   | μ.<br>" |
| 5.0 "     | 7.0 (5)                                | 5.0—9.0       | 7.0     | "                     | "         | "       |
| 6.0 "     | 15.5 (10)                              | 7.0—22.5      | 14.75   | "                     | "         | "       |
| 7.0 "     | 33.0 (8)                               | 22.5—49.5     | 36.0    | "                     | "         | "       |
| 8.30 "    | 88.0 (10)                              | 54.0—157.5    | 105.5   | 9 (8)                 | 4.5—20.0  | 12.0    |
|           | Too long to measure, but 300 to 500 μ. | about         | about   | 45 (8)                | 9.0—85.5  | 47.5    |

Germination had not begun at 3.10, but was commencing at 4.30 in the red, whereas it was delayed till 8.30 in the blue, and would, no doubt, have been still more tardy at a lower temperature.

A review of the foregoing results where no glass was used (except the thin cover slips in some) only confirms the previous results.

1. The spores are distinctly retarded or killed by five or six hours' exposure to daylight, even of low intensity, quite apart from temperature.

2. The *growing* filaments are often not measurably retarded within the period observed, except under conditions such as do not exclude possible temperature effects; but the evidence goes to show that the light slowly retards the growth, acting like a brake on the curve of growth.

3. Further testing of the measurements and curves on growing filaments under like conditions, confirms the confidence in their accuracy, and they may be accepted as very good approximations.

4. Some of these measurements bring out clearly the extreme sensitiveness to changes of temperature of the growing filaments, and emphasise clearly how difficult it is to avoid this source of error.

5. The mode of action of the light may be conceived of in several ways, keeping in view the differences of effect on spores and filaments. First, we might suppose it promotes oxidations in the surrounding food materials, resulting in the formation of poisonous substances which kill the spores but not the filaments; this (taking into account the resistance of the spores to physical agencies, and the evidence previously given) seems unlikely, for unless the *living protoplasm of the actively growing cells* has some extraordinary power of destroying such poisons as fast as they are made, whereas the *dormant protoplasm of the resting spore* is incapable of this, it seems incredible that the spores, otherwise so highly resistant, should succumb more easily than the otherwise so slightly resistant filaments. Secondly, we might suppose that the light action takes effect directly on some easily destroyed reserve material in the spore, which does not exist as such in the actively metabolising growing cell. This would explain the retardation of germination, or the death of the spores, according to the amount of destruction of the spore contents, very well; but it is not easy to accept the assumption that the light is totally without effect on the growing cell. A third possibility seems to be that the light action makes itself effective in promoting some intense metabolic activity in the spores and growing cells alike, and which is connected with enhanced respiration. In this case we might suppose the *spores* to suffer from the too rapid consumption of their unstable reserve materials (as before), while the growing cells do not show the effects *so long as plenty of food material is still available* in the hanging drop, and, therefore, so long as the filaments are still measurable. At a later period, however, the overworked machinery results in the production of much feebler plants, capable of developing a few poor spores only, or even none at all, such as was observed in several cases where the cultures were allowed to go on. Here, then, we can understand why the direct measurements often give no decisive results: the actively growing filaments can only be measured during a short period, but the *cumulative effect of the malnutrition is not evinced by diminished growth until after the measurements have ceased, or at least till towards the end of the period.*\*

6. But if the latter hypothesis is accepted, we have to recognise

\* This is decidedly against action on the food materials, since one would expect the effect to make itself evident very early in the growth as the injurious bodies reach the sensitive cells.

that the fundamental physiological function affected by the light is *not growth*, but *nutrition*—constructive metabolism; and this would coincide very well with what Elfving found to be the case in macroscopic cultures of fungi.

7. However, the possibilities are not exhausted by the above. There are several points which suggest that the destructive light action may take place *on the enzymes* which the living cell forms and excretes. If Green is right in concluding that the more refrangible rays\* are destructive to the action of enzymes outside the cell, it may be that we have here the key to the mystery, and that the cells gradually die of inanition from inability to render their food materials assimilable. Still the experiments throw no light on whether such action takes place outside or inside the cell,† though, perhaps, the results with spores support the latter idea rather than the former one.

8. Whatever the light action consists in, it is evidently exerted by the more refrangible rays, and is the more pronounced the more intense the light, or with feeble lights the greater the proportion of these rays there are in it.

9. The evidence goes to show also, that the rays at the other end of the spectrum, and especially the heat rays in the red and below, co-operate in the light action in question (or, possibly, sometimes antagonise it) in various ways. The probability is that with a given moderate intensity of light, such as occurs in ordinary daylight, the damaging effect of the blue-violet rays on growing cells is dependent on the temperature. If the temperature is about the optimum, the protoplasm, working at its best, seems able to resist—perhaps even undo—the damage; if the temperature is far removed from the optimum, however, the injurious action of the light rays is cumulative, and results in more or less rapid retardation of growth, and eventual death. This is, no doubt, true, whether the protoplasm fails to combat the injury because too *passive*, as at low temperatures, or because over-stimulated and too *active*, as at high temperatures. Taking into account all we know, however, it seems improbable that the organism can resist the bactericidal action of the more refrangible rays *at any temperature*, if those rays are relatively abundant in an intense light.

\* Green—"The Influence of Light on Diastase" ('Annals of Botany,' 1894, pp. 370—373).

† It is not inconceivable that when the enzymes begin their work in the food material, the products of their action are easily oxidised. Indeed some experiments point to the probability of this, and suggest that broth with enzymes in it is susceptible to some destruction.

*Experiments with the Light of the Electric Arc, and others.*

In order to test the validity of previous conclusions as to the essential similarity of action of the electric and solar light, I started the following experiments. In spite of the kindness of my colleagues, Professor Stocker and Mr. Shields, in lending me the apparatus and use of a dark room, and the assistance of Mr. West, who took a lot of trouble in helping me, I had to abandon these experiments, partly because the lamp was not sufficiently powerful, and partly owing to the close attention and long periods of watching they require, and also because of other difficulties. I carried them sufficiently far, however, to show that the matter is worth further attention, and append the results in the hope that someone will take up this line of investigation.

On July 13 I tried the effect of exposing spores, in weak-gelatine, over the water and the bichromate screens, to the electric arc-light, reflected up from silvered mirrors.

The lamp used was a small, old-fashioned Dubosq, and the microscopes, with shades, match-box screens, quartz, and controls, &c., arranged exactly as before, were 3 ft. from the arc. The current used was equal to 8 ampères, and was taken from 32 storage cells. No reflectors or lenses were used, and much light was lost.

The spores were sown at 3 P.M., and the exposures began at 3.15.

The temperatures remained the same, or varied similarly, in both cultures, and during the whole period of exposure—3 hours—only rose from 18° C. to 19.25° C.

On stopping the experiment at 6.15 P.M., the cultures were at once placed in the dark, side by side, at 18° C. The temperature slowly fell to 16° C. at 10.45, when no results were observed.

At 7 A.M., on the 14th July, the bichromate culture showed rodlets and filaments 250  $\mu$  long and upwards; but the culture over water did not show a single germinated spore.

I was not entirely satisfied with this result, however, because the gelatine drop had run a little, owing to condensation of water, and therefore regarded the result as negative.

On July 14 this experiment was repeated exactly, the spores being sown at 9 A.M. and exposed at 9.15 A.M. The spectra of the screens were carefully examined, and everything arranged as exactly as possible.

The temperatures were as follows:—

| Time.      | Bichromate.<br>°C. | Water.<br>°C. |
|------------|--------------------|---------------|
| 9.15 A.M.  | 17.5               | 17.5          |
| 9.55 „     | 18.0               | 18.0          |
| 10.40 „    | 18.5               | 18.5          |
| 11.30* „   | 19.0               | 19.0          |
| 11.55 „    | 19.0               | 19.0          |
| 12.20 P.M. | 19.0               | 19.0          |

At 12.20 the cultures were put into the dark incubator at 22° C., and examined occasionally during the afternoon, with the following results.

At 3 P.M. there were several germinating rodlets in the bichromate culture, measuring from 4.5  $\mu$  to 15.75  $\mu$ ; but none could be found in the water-culture more than 2  $\mu$  long—swelling spores, in fact.

At 4.30 P.M., however, no differences of importance between the two cultures could be detected. I measured 15 rodlets—all that could be found—in the bichromate culture, and these ranged from 5  $\mu$  to 64  $\mu$ ; while fourteen measurements in the water culture gave from 4.5  $\mu$  to 54  $\mu$ , a difference that could not be insisted upon.

It must be concluded from this that the above exposure (3 hours) has no marked effect on the spores. It is, perhaps, worth mention that a reflection of the western sun, on a hazy day (July 13) thrown from a plane mirror on to the disc of light given by the lamp at about 10 ft. was much brighter than the disc.

After the foregoing, it need hardly be said that 1½ hours' exposure—experiment from 1.35 to 3.5 P.M. on July 14—gave no results.

On July 16, with the same arc, but at 2 ft. distance, I exposed two cultures, arranged exactly as before, for *six* hours—viz., from 9.35 A.M. to 3.45 P.M.

The temperature slowly rose from 16.75 (bichromate) and 17.25 (water) at 9.35 to 18 in both at 9.50; to 20° C. at 12.50, and finally to 21 at 3 P.M., but never passed beyond that.

At 3.50 the cultures were placed in the dark incubator at 22° C. Neither showed any signs of growth up to 10.30 P.M., but at 12 noon, on the 17th July, the culture over water showed one weak tuft of filaments, the longest being 140 to 180  $\mu$  long; those in the bichromate culture were 675 to 800  $\mu$  long.

This indicates a feeble light action, no doubt; but it is remarkable that the bichromate culture had not made more progress in the time!

On July 26, with exactly similar arrangements, two broth drop-cultures were exposed, from 9.30 A.M. to 3.30 P.M., a full six hours. The temperature rose from 19 at 9.30, to 21.5 at noon, and to 22 at 1 P.M., and so to the end.

\* Five minutes were occupied in replacing the carbons.

Before removing to the incubator, at 24° C., I examined both carefully. No trace of a rodlet could be found in the water culture, but 30 or 40 filaments from 200 to 300 and even 450  $\mu$  were abounding in that over bichromate.

At 7 P.M., re-examination showed immeasurably long filaments in the bichromate culture, whereas only three filaments 58, 96, and 81  $\mu$  long respectively could be found in the water culture.

I think this must be regarded as proof that light-action occurs under the above conditions of exposure; and there can be little doubt that with a more powerful lamp—perhaps aided by quartz condensers, &c.—effects more nearly approaching those with sunlight could be obtained.

#### *Antagonistic Action of a Swarming Bacillus.*

The following experiment illustrates very clearly the effect of introducing a rapidly growing, strongly aerobic bacillus to compete for oxygen, &c., with the schizomycete in question.

I selected a well grown culture of *B. ramosus*, and introduced into the broth-drop a few rodlets of perhaps the most rapid growing liquefying bacillus yet isolated from the Thames.

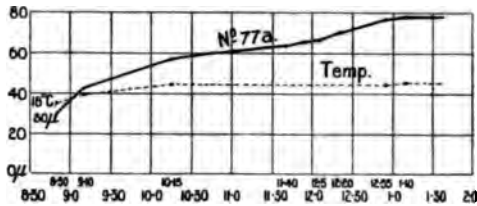
This form belongs to the group commonly known as *B. termo*, and its peculiarities will be described in due course. I call it, for the present, form  $\beta$ . It is a short, stout rodlet, about 1.5 to 2  $\mu \times 1 \mu$ , actively motile, a very rapid grower, and exceedingly greedy of oxygen.

At 8.50 A.M., a portion of the *B. ramosus*, around which the bacillus  $\beta$  was rapidly swarming in considerable numbers, was put into measurements, with the following suggestive results :—

| Time.     | Length. | Growth. | Interval. | Rate.   | Temp.<br>(air). |
|-----------|---------|---------|-----------|---------|-----------------|
|           | $\mu$ . | $\mu$ . | mins.     | $\mu$ . | ° C.            |
| 8.50 A.M. | 30      | —       | —         | —       | 18.0            |
| 9.10 "    | 42      | 12      | 20        | 0.6     | 20.0            |
| 10.15 "   | 58      | 16      | 65        | 0.25    | 22.0            |
| 11.40 "   | 64      | 6       | 25        | 0.24    | 22.0            |
| 11.55 "   | 66      | 2       | 15        | 0.13    | 22.0            |
| 12.5 P.M. | 67      | 1       | 10        | 0.10    | 22.0            |
| 12.20 "   | 70      | 3       | 15        | 0.20    | 22.0            |
| 12.55 "   | 77      | 7       | 25        | 0.3     | 22.0            |
| 1.10 "    | 78      | 1       | 15        | 0.07    | 22.5            |
| 1.20 "    | 78      | 0       | 10        | 0.0     | 22.5            |

Curve 77A.

It doubled its length in 115 minutes at (air-temp.) 18—22° C.



The growth had entirely ceased, as I convinced myself by nursing the culture in the incubator for the rest of the day.

Whether this was due entirely to the loss of oxygen brought about by the form  $\beta$ , which showed beautiful chemotactic aggregations about the filaments, by the bye—or to other causes brought about by the action of the intruder, cannot be with certainty determined, but it is obviously a line of enquiry worth further pursuit. In any case the shape of the curve is entirely different from the usual one, and fully supports the conclusion that the intruding bacterium was robbing the filaments of oxygen.

The observation is of considerable interest as touching the questions of antagonism and chemotaxis, and it is probable that the subject would well repay investigation. I had to refrain from pursuing the matter further, however, and merely give this case—not the only one of the kind that has come under my notice—owing to the necessity of following the special line of research I was engaged in.

#### *Effect of probable Poisonous Action.*

The following is quoted as probably a case of the poisonous action of some product of combustion of the over-heated cotton-wool in the arms of the cell. It is not *certain* that the action was what I suppose, but it is suggestive that all the filaments in the drops were suffering, whence may be safely inferred that some common action was going on.

On November 11, spores were sown exactly as before, in normal gelatine, at 10.45, at 22° C.: at 4 P.M. the measurements were started at 25° C.

| Time.    | Length.                                                                 | Interval. | Growth. | Rate.   | Temp.                |
|----------|-------------------------------------------------------------------------|-----------|---------|---------|----------------------|
|          | $\mu$ .                                                                 | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . |
| 4.0 P.M. | 18.0                                                                    | —         | —       | —       | 25.0                 |
| 4.15 "   | 24.75                                                                   | 15        | 6.75    | 0.44    | 25.0                 |
| 4.30 "   | 31.5                                                                    | 15        | 6.75    | 0.44    | 25.0                 |
| 4.45 "   | 36.0                                                                    | 15        | 4.5     | 0.3     | 25.0                 |
| 5.0 "    | 42.75                                                                   | 15        | 6.75    | 0.44    | 25.1                 |
| 5.11 "   | 49.5                                                                    | 11        | 6.75    | 0.61    | 25.25                |
| 5.22 "   | 51.75                                                                   | 11        | 2.25    | 0.2     | 25.3                 |
| 5.30 "   | 55.0                                                                    | 8         | 3.25    | 0.4     | 25.3                 |
| 5.40 "   | } Filament = pathological contraction and abnormal behaviour—see below. |           |         |         | 25.4                 |
| 5.45 "   |                                                                         |           |         |         | 25.5                 |
| 5.50 "   |                                                                         |           |         |         | 25.5                 |
| 6.5 "    |                                                                         |           |         |         | 25.3                 |
| 6.16 "   | 72.0                                                                    | 11        | 6.75    | 0.61    | 25.3                 |

Stopped, because evidently abnormal. Doubling periods:—

18—36  $\mu$  = 4.0 P.M.—4.45 P.M. = 45 minutes at  $25^{\circ}\text{C}$ .

36—72 " = 4.45 " — 6.16 " = 91 " 25—25.5—25.3  $^{\circ}\text{C}$ .

Normal doubling period\*:—

31.5—63  $\mu$  = 4.30 P.M.—5.59 P.M. = 89 minutes at 25—25.5—25.3  $^{\circ}\text{C}$ .

A very curious phenomenon occurred here. At 5.40 the filament had ceased growing, and apparently measured only 49.5  $\mu$ —whereas it was 55  $\mu$  ten minutes previously. More careful observation showed a length of *empty sheath* 9  $\mu$  long in addition, so that it was really 58.5  $\mu$  long, but the solid part had *contracted* itself. The cause of this is not evident, and as the table shows, the temperature, &c., could not have been responsible; but the phenomenon is apparently of the same order as the abnormality described on p. 356 by exposure to intense insolation. At 5.45 the solid part was 54  $\mu$  long, and at 5.50 it was 58.5  $\mu$ . By 6.5 the empty portion had partly filled up again by a block separated by a piece of empty sheath (about 1  $\mu$  long) from the main mass; by 6.15 the abnormality seemed quite repaired.

I have no idea what could have induced this malformation: there is an interesting question involving the maladies of a schizomycete here, but I could only note it in passing.

Its bearing on my present purpose is evident. The disturbance caused the filament to take ninety-one minutes to double its length against forty-five in the previous period; evidently one cause of this would be the loss of cell-divisions and their summation.

\* See p. 431 for explanation of the normal doubling period—it is obtained by dating the commencement of the measurements half an hour after starting the growth, to allow the cell time to accommodate itself to the temperature.

There was no reason for concluding that the main mass of the filament grew any slower, but it may have been so.

It scarcely seemed worth while to pursue this, so another filament was taken in the same culture.

The very slow growth, although at and near 25° C., was confirmed by examining another culture, started at the same time at 22° C., and put into Sachs' box at 6.30 P.M.

| Time.     | Length. | Interval. | Growth. | Rate. | Temp. |
|-----------|---------|-----------|---------|-------|-------|
|           | μ.      | mins.     | μ.      | μ.    | ° C.  |
| 6.43 P.M. | 58.5    | —         | —       | —     | 24.25 |
| 6.52 "    | 65.25   | 9         | 6.75    | 0.7   | 26.25 |
| 7.4 "     | 69.75   | 12        | 4.5     | 0.4   | 26.1  |
| 7.19 "    | 74.25   | 15        | 4.5     | 0.3   | 25.75 |

Here the curve was actually flattening more and more as time went on.

Another filament was then taken.

| Time.     | Length. | Interval. | Growth. | Rate. | Temp. |
|-----------|---------|-----------|---------|-------|-------|
|           | μ.      | mins.     | μ.      | μ.    | ° C.  |
| 8.17 A.M. | 56.25   | —         | —       | —     | 24.75 |
| 8.37 "    | 63.0    | 20        | 6.75    | 0.3   | 25.0  |
| 9.11 "    | 76.5    | 34        | 13.5    | 0.4   | 25.0  |
| 9.24 "    | 81.0    | 13        | 4.5     | 0.3   | 25.0  |

The growth was so slow that I abandoned the measurements. I cannot explain it, unless the following suggestion is to the point. The cotton-wool plugging the arms had been slightly scorched during sterilisation: my practice is not to heat up to browning point, but in this case that had occurred. Can it be that traces of pyridine-like or other bodies get into such a cell, and cause slight antiseptic inhibition? It would probably be worth somebody's while to make a series of experiments with such cultures to this end, with various volatile bodies added in known doses in the arms of the cells.

#### *Experiments on the Effects of Temperature only.*

Various results in the foregoing account of my experiments suggested that it would be well to obtain more accurate information as to the effect of temperature alone on the growth curves. Of course a good deal of work has been done on the influence of temperature

generally, on the growth of bacteria, so far as their *macroscopic* characters are concerned, and a few observers—notably Brefeld with *B. subtilis*—have made observations on the rate of cell-division at one or two principal air-temperatures; but, so far as I know, no one has attempted to measure quantitatively and in detail the effect of small differences of temperature over a long range, and that temperature *that of the culture*—which we have seen *may* be different from that of the air—and especially to obtain plotted out curves of growth under such conditions.

It is obvious that my methods admit of this being done, and I thought it might be not only productive of useful information as to the primary point concerned—the definite effect of given temperatures on the growth—but that I might possibly be able to make use of these *normal* temperature-curves indirectly, by comparing them with the foregoing.

I accordingly started what proved to be a very long and laborious series of cultures under a microscope enclosed in an incubator of an improved form modelled on the original type described some years ago by Sachs. The chief difference is that the heating arrangement is a thick, flanged, blackened, iron plate, on which stands a thick sheet of asbestos-board, and on this the microscope. The whole body of the microscope, except the eye-piece and micrometer screw, is enclosed in a wooden box, of which the iron plate is the floor; this box has a glass window in front and two lateral openings (with shutters) for manipulating the culture, and can easily be opened entirely if necessary.

A culture being placed in position, and the microscope focussed on a selected rodlet or filament, the shutters are all closed and the growth goes on in the dark.

The temperature is registered by a thermometer whose bulb is inside and reading column projecting through the roof: the gas passes through a very delicate thermo-regulator, containing mercury and ether, and when once the apparatus (which I shall henceforth term Sachs' box) is heated up, it can easily be kept at a temperature so constant that it does not alter a degree in several hours, while, with a little more care (and provided no large alterations of temperature are going on in the room), it will remain for hours within  $0.25^{\circ}$  C. of the temperature arranged for.

Of course, the temperature falls or rises quickly when the box is opened—unless it is that of the room—but it returns in five to ten minutes if the opening and shutting are completed in a minute or so; the opening and shutting of the side windows, and the removal and replacement of the shutter during observations, produce very slight slowly acting effects, which, however, cannot be neglected, as we shall see.

The ideal is practically realised in the case of a culture which, once in position, remains without the necessity of any opening at all, and this can frequently be carried out. In order to meet obvious objections, however, I have in all cases made several cultures at each temperature, and to make the results strictly comparable, I adopted the following method of recording.

1. The table of growths was prepared, giving the time of observation, temperature, and length of filament, &c., as in preceding cases.

2. Then the curve of growth was plotted out on sectional paper, care being taken that the squares, &c., were all equal, and that the intervals between the observations were sufficiently short and numerous to give good curves. Since the measurements were all made with the same micrometer-scale and the same microscope, and taking into account the proofs of accurate measurement already given, no more need be said on that head.

3. From these curves and tables I then measured the period occupied by a rodlet or filament of any given length (to start with) in growing to *double its length*, and call this the *doubling period*.

4. The average *doubling period* for any temperature is then obtained in the usual way, by taking the sum of the times and dividing by the number of observations. Before saying more, however, it will be best to examine the actual results, which now follow.

I may add that I had already satisfied myself that the curves obtained at widely different temperatures, are markedly different, and less and less divergent as the temperatures of growth approach one another, facts which are in accordance with experience with other organisms, and which will be evident enough as we proceed.

In what follows I select a number of representative curves from larger series made to familiarise myself with the details; and for the sake of classification—and with reference to some conclusions later—I present them arranged according to the food-materials employed. The media chiefly used were four, viz.: (1) normal beef-broth; (2) the same with 1 per cent. of gelatine added to give a certain degree of stiffness; (3) a stiff 10 per cent. gelatine with mere traces of broth (referred to as weak gelatine); and (4) normal 10 per cent. broth-peptone gelatine.

For various reasons I shall start with the cultures in weak gelatine.

#### *Cultures in Weak Gelatine.*

On July 19 spores were sown in the stiff weak gelatine used throughout July hitherto, and put into Sachs' box at 7 P.M. at 17° C. The temperatures ran as follows:—

| Time.     | Temp.<br>° C.        |
|-----------|----------------------|
| 7.0 P.M.  | = 17.0               |
| 8.30 „    | = 16.5               |
| 8.30 A.M. | = 16.5 (germination) |
| 11.15 „   | = 17.25              |
| 11.50 „   | = 17.5               |
| 12.20 „   | = 18.5               |

And now it was possible to begin the measurements, which ran as follows:—

| Time.      | Length. | Interval. | Growth. | Rate.   | Temp. |
|------------|---------|-----------|---------|---------|-------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 12.30 P.M. | 9.0     | —         | —       | —       | 18.5  |
| 1.45 „     | 13.5    | 85        | 4.5     | 0.05    | 18.5  |
| 2.45 „     | 18.0    | 60        | 4.5     | 0.07    | 18.0  |
| 3.30 „     | 23.75   | 45        | 5.75    | 0.12    | 18.0  |
| 4.0 „      | 27.0    | 30        | 3.25    | 0.11    | 18.0  |
| 4.30 „     | 31.5    | 30        | 4.5     | 0.15    | 18.0  |
| 5.0 „      | 38.25   | 30        | 6.75    | 0.22    | 17.9  |
| 5.30 „     | 45.0    | 30        | 6.75    | 0.22    | 17.75 |
| 6.0 „      | 54.0    | 30        | 9.0     | 0.3     | 17.8  |
| 6.30 „     | 65.25   | 30        | 11.25   | 0.37    | 17.8  |
| 6.50 „     | 72.0    | 20        | 6.75    | 0.34    | 17.9  |
| 7.30 „     | 88.0    | 40        | 16.0    | 0.4     | 18.0  |
| 8.45 „     | 117.0   | 85        | 29.0    | 0.34    | 17.25 |
| 9.5 „      | 130.5   | 20        | 13.5    | 0.67    | 17.25 |
| 9.20 „     | 135.0   | 15        | 4.5     | 0.3     | 17.4  |
| 9.40 „     | 139.5   | 20        | 4.5     | 0.22    | 18.0  |
| 8.45 A.M.  | 1100.0  | —         | —       | —       | 18.0  |

Reckoned back:—

139.5 — 69.75  $\mu$  = 6.43 P.M. — 9.40 P.M. = 177 mins. at 17.8—18.0—17.25—18 °C.

69.75 — 34.75 „ = 4.44 „ — 6.43 „ = 119 „ 18.0—17.8 °C.

34.75 — 17.5 „ = 2.40 „ — 4.44 „ = 124 „ 18.0—° C.

The doubling periods were as follows:—

1. 9—18  $\mu$  = 135 minutes at 18.5—18 °C.
2. 18—36 „ = 120 „ 18.0—17.9 °C.
3. 36—72 „ = 125 „ 18.0—17.75—17.9 °C.
4. 72—144 „ = 170 (approx.) at 17.9—18—17.4—18 °C.

partly calculated.

Normal doubling period:—

10—20  $\mu$  = 12.50 P.M. — 3.4 P.M. = 134 minutes at 18.5—18 °C.

From observation next morning, and what was seen during the slowing after 7.30, there can be little doubt the inhibition was partly

due to the increasing stiffness of the gelatine at the low temperatures, and to the difficulty the feeble growth had in peptonising so weak and stiff a medium, and partly to the lack of oxygen consequent on congelation.

On July 17 spores were sown as before, and at once put at 23° in the Sachs' box: this was 8.30 A.M. The temperatures during the morning, &c., ran as follows:—

| Time.        | Temp.<br>° C.           |
|--------------|-------------------------|
| 8.30 A.M. =  | 23.0                    |
| 9.10 „ =     | 23.75                   |
| 9.30 „ =     | 23.25                   |
| 10.5 „ =     | 23.2                    |
| 11.50 „ =    | 23.25                   |
| 12.20 P.M. = | 23.25                   |
| 12.40 „ =    | 23.5                    |
| 1.30 „ =     | 24.0                    |
| 1.35 „ =     | 22.8                    |
| 1.40 „ =     | 22.75                   |
| 1.55 „ =     | 23.0                    |
| 2.10 „ =     | 24.0                    |
| 3.15 „ =     | 24.0                    |
| 3.16 „ =     | 23.0 (opened box)       |
| 4.20 „ =     | 23.5                    |
| 5.5 „ =      | 23.0                    |
| 5.12 „ =     | 22.0 (opened side door) |

I now began the measurements, and continued them till 8.15 P.M. In all these cases where only the doubling periods are given, it must be understood that the table of growths was prepared in detail, and the curve plotted. The tables and curves are too numerous to give in full.

The doubling periods were as follows:—

1. 31.5—63  $\mu$  in 56 minutes at 23.1—22.7° C.
2. 63.0—126 „ „ 53 „ 22.7—24.25° C.

Normal doubling period:—

50.5—101  $\mu$  = 6 P.M.—6.51 P.M. = 51 minutes at 23—22.7—23.1° C.

On July 11 I arranged for a culture at 25° C., approximately constant temperature, in Sachs' chamber. Spores were sown at 9.30 in weak gelatine,\* and at once put in, though the chamber was not

\* Weak gelatine here and throughout = a stiff gelatine (10 per cent.) with only 0.5 per cent. of broth.

as yet low enough in temperature. Germination began about noon, the temperatures meanwhile falling as follows:—

| Time.      | Temp.<br>° C.                                                                                                |
|------------|--------------------------------------------------------------------------------------------------------------|
| 9.30 A.M.  | = 28.5                                                                                                       |
| 9.45 "     | = 29.0                                                                                                       |
| 10.10 "    | = 26.25                                                                                                      |
| 10.20 "    | = 25.5                                                                                                       |
| 10.30 "    | = 25.25                                                                                                      |
| 10.45 "    | = 25.0                                                                                                       |
| 11.5 "     | = 25.0 (fell to 24.25° on opening to<br>arrange culture)                                                     |
| 11.15 "    | = 25.0                                                                                                       |
| 11.45 "    | = 25.0                                                                                                       |
| 12 noon    | = 25.0                                                                                                       |
| 12.15 P.M. | = 25.0                                                                                                       |
| 12.30 "    | = 25.0                                                                                                       |
| 1.30 "     | = 25.3                                                                                                       |
| 1.35 "     | = 25.0 (at 1.55 had to open box,<br>and then fell to 22.5°. At<br>1.57 = 23°, 2 P.M. = 24°,<br>2.15 = 24.5°) |
| 2.30 "     | = 25.0                                                                                                       |

At 2.45 I was able to start the measurements on a vigorous rodlet then selected.

The Sachs' chamber was kept dark by a tinfoil curtain, and the following table shows how constant the temperatures were:—

I had now to abandon this, owing to its breaking up and being too difficult to measure.

Another filament, part of this longer one, was therefore selected, with results given in the following table and curve:—

| Time.     | Length. | Interval. | Growth. | Rate. | Temp. |
|-----------|---------|-----------|---------|-------|-------|
|           | μ.      | mins.     | μ.      | μ.    | ° C.  |
| 2.45 P.M. | 18.0    | —         | —       | —     | 24.5  |
| 3.15 "    | 35.0    | 30        | 17.0    | 0.57  | 24.5  |
| 3.30 "    | 42.75   | 15        | 9.75    | 0.65  | 24.75 |
| 3.45 "    | 54.0    | 15        | 11.25   | 0.75  | 25.0  |
| 4.0 "     | 67.5    | 15        | 13.5    | 0.9   | 25.5  |
| 4.15 "    | 83.25   | 15        | 15.75   | 1.05  | 25.5  |
| 4.30 "    | 105.75  | 15        | 22.5    | 1.5   | 25.0  |
| 4.40 "    | 121.5   | 10        | 15.75   | 1.57  | 25.0  |
| 4.50 "    | 139.5   | 10        | 18.0    | 1.8   | 25.0  |
| 5.0 "     | 159.75  | 10        | 20.25   | 2.0   | 25.0  |
| 5.10 "    | 184.5   | 10        | 24.75   | 2.47  | 25.25 |

On analysing the results, the filament doubled its length, as follows :—

1. 18— 36  $\mu$  in 32 minutes at 24·5° C.
2. 36— 72 „ „ 47 „ 24·75—25·5° C.
3. 72—144 „ „ 43 „ 25·5—25° C.

Reckoned back :—

184·5 —92·25  $\mu$  = 4.21 P.M.—5.10 P.M. = 49 minutes at 25·5—25—25·25° C.  
 92·25—46·0 „ = 3.35 „ —4.21 „ = 41 „ 24·75—25·5—25° C.  
 46·0 —23·0 „ = 2.53 „ —3.25 „ = 32 „ 24·5—25° C.

Normal doubling periods :—

35— 70  $\mu$  = 3.15 P.M.—4.3 P.M. = 48 minutes at 24·5—25·5° C.  
 70—140 „ = 4.3 „ —4.50 „ = 47 „ 25·5—25° C.

The following are the results of the second series of measurements ; the slight variations in the temperature (not to be overlooked) were due to the necessity of rearranging the culture, and therefore opening the side windows.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 5.24 P.M. | 81·0    | —         | —       | —       | 25·0  |
| 5.30 „    | 87·75   | 6         | 6·75    | 1·12    | 24·75 |
| 5.40 „    | 99·0    | 10        | 11·25   | 1·12    | 24·75 |
| 5.50 „    | 114·75  | 10        | 15·75   | 1·57    | 25·0  |
| 6.0 „     | 126·0   | 10        | 11·25   | 1·12    | 25·25 |
| 6.10 „    | 144·0   | 10        | 18·0    | 1·8     | 25·0  |
| 6.20 „    | 157·5   | 10        | 13·5    | 1·35    | 25·25 |
| 6.30 „    | 173·25  | 10        | 15·75   | 1·57    | 24·75 |
| 6.40 „    | 198·0   | 10        | 24·75   | 2·47    | 25·0  |
| 6.50 „    | 211·5   | 10        | 13·5    | 1·35    | 25·0  |
| 7.0 „     | 231·75  | 10        | 20·25   | 2·02    | 25·2  |
| 7.10 „    | 252·0   | 10        | 20·25   | 2·02    | 25·0  |

Calculated back :—

252—126  $\mu$  = 6 P.M.—7.10 P.M. = 70 minutes at 25·25—24·75—25·2—25° C.

The doubling, from 81  $\mu$  to 162  $\mu$ , took 59 minutes at temperatures varying between 24·75° and 25·25° C., and, by the shape of the curve, the rate would probably not be far different for the next doubling period.

This specimen, which had grown vigorously during the night of the 11th, was kept at the same temperature, 24·5° to 25·5°, all through the 12th, in the dark. It was necessary to open the chamber at 9.30 A.M. on the 12th, and the temperature fell to 22·5° for a few minutes, but it had recovered before 10 A.M.

At 9 P.M. it had completed the formation of magnificent series of spores, *i.e.*, in less than 36 hours from sowing, the sporification was complete.

On July 15 I repeated the observations in the Sachs' box, the sowing being made—weak gelatine—and put in at 12 noon.

The temperatures of the box ran as follows, before germination :—

| Time.      | Temp.<br>° C. |
|------------|---------------|
| 10.0 A.M.  | = 23·5        |
| 10.30 „    | = 24·25       |
| 11.0 „     | = 24·75       |
| 11.30 „    | = 25·2        |
| 12 noon    | = 25·25       |
| 12.15 P.M. | = 24·75       |
| 12.30 „    | = 25·25       |
| 3.15 „     | = 26·75       |
| 4.0 „      | = 26·75       |
| 5.15 „     | = 26·75       |
| 7.40 „     | = 27·75       |

The measurements began at 7.45, and continued till 10.10 P.M., with the following results :—

Doubled its length as follows :—

1. 36—72  $\mu$  in 50 minutes at 28—27·25° C.
2. 72—144 „ „ 58 „ „ 27·25—25·5° C.

And at its present rate would have again doubled :—

3. 144—288  $\mu$  in 54 minutes at 25·5° C.

Normal doubling periods :—

- 55—110  $\mu$  = 8.15 P.M.—9.10 P.M. = 55 minutes at 27·5—26° C.
- 110—220 „ = 9.10 „ —10.6 „ = 56 „ „ 26·0—25·5° C.

At 9 P.M. on the 16th, the culture having been at 25° to 27·5° during that day (and up to 30·5° between 6.20 and 8.30 P.M.), many of the filaments had already formed their spores, *i.e.*, spore-formation had definitely set in in 33 hours. At 8.30 next morning there were fully formed spores in all filaments, large, and very brilliant.

It is clear that at temperatures near 25—27° C. spores may be developed in less than 48 hours.

This experiment was repeated on the 16th July. The spores, exactly as before, were started at 9.45° C., the temperatures running as follows :—

| Time.      | Temp.<br>° C. |
|------------|---------------|
| 9.45 A.M.  | = 24.75       |
| 9.50 "     | = 25.5        |
| 10.15 "    | = 25.6        |
| 10.55 "    | = 25.9        |
| 11.25 "    | = 26.25       |
| 11.40 "    | = 26.4        |
| 12.15 P.M. | = 26.5        |
| 1.30 "     | = 26.75       |
| 2.10 "     | = 27.0        |

Meanwhile, germination had occurred, and the measurements were now begun on a germinal rodlet  $9\ \mu$  long, and still with its end in the spore.

| Time.     | Length.      | Interval. | Growth. | Rate.   | Temp. |
|-----------|--------------|-----------|---------|---------|-------|
|           | $\mu$ .      | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 2.10 P.M. | 9.0          | —         | —       | —       | 27.0  |
| 2.55 "    | 18.0         | 45        | 9.0     | 0.2     | 26.75 |
| 3.20 "    | 27.0         | 25        | 9.0     | 0.36    | 26.5  |
| 3.40 "    | 36.0         | 20        | 9.0     | 0.45    | 26.6  |
| 4.0 "     | 47.25        | 20        | 11.25   | 0.56    | 26.9  |
| 4.20 "    | 65.25        | 20        | 18.0    | 0.9     | 27.1  |
| 4.30 "    | 77.5         | 10        | 12.25   | 1.2     | 27.2  |
| 4.40 "    | 90.0         | 10        | 12.5    | 1.25    | 27.25 |
| 4.50 "    | 105.75       | 10        | 15.75   | 1.57    | 27.4  |
| 5.0 "     | 121.5        | 10        | 15.75   | 1.57    | 27.5  |
| 5.10 "    | 137.25       | 10        | 15.75   | 1.57    | 27.5  |
| 5.20 "    | 157.5        | 10        | 20.25   | 2.0     | 27.6  |
| *5.30 "   | 186.75       | 10        | 29.25   | 2.9     | 27.7  |
| 5.40 "    | 204.75       | 10        | 18.0    | 1.8     | 27.5  |
| 5.50 "    | 225.0        | 10        | 20.25   | 2.0     | 27.4  |
| 6.0 "     | 254.25       | 10        | 29.25   | 2.9     | 27.5  |
| 6.10 "    | 279.0        | 10        | 24.75   | 2.47    | 27.6  |
| 6.20 "    | 315.0        | 10        | 36.0    | 3.6     | 27.75 |
| 8.30 "    | Immeasurable | —         | —       | —       | 30.5  |

Curve 83, p. 402.

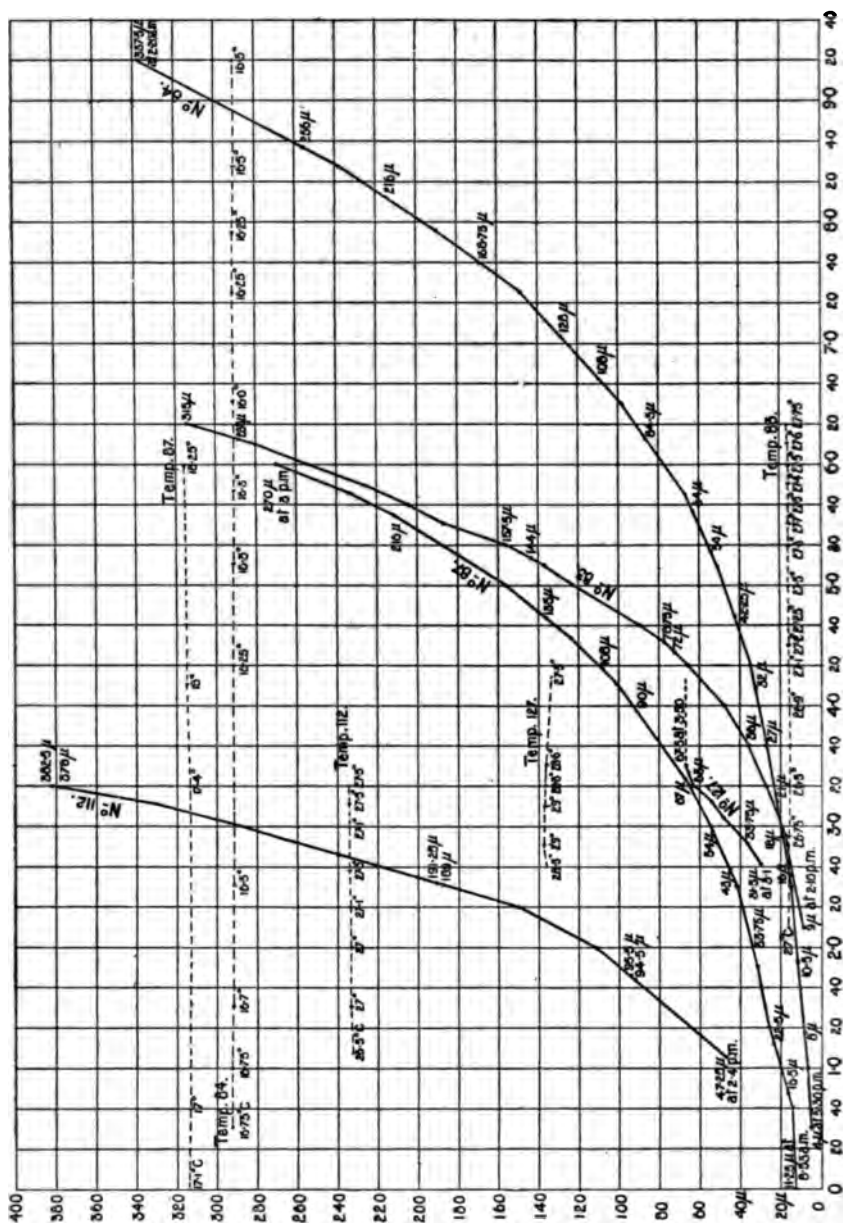
The doubling occurred as follows:—

1. 9—18  $\mu$  = 2.10 P.M.—2.55 P.M. = 45 minutes at 27—26.75° C.
2. 18—36 " = 2.55 " —3.40 " = 45 " 26.75—26.6° C.
3. 36—72 " = 3.40 " —4.26 " = 46 " 26.6—27.1° C.
4. 72—144 " = 4.26 " —5.14 " = 48 " 27.1—27.6° C.
5. 144—288 " = 5.14 " —6.12 " = 58 " 27.6—27.4—27.75° C.

Reckoned back:—

- 315—157.5  $\mu$  = 5.20 P.M.—6.20 P.M. = 60 minutes at 27.6—27.7—27.4—27.75° C.
- 157.5—78.75 " = 4.31 " —5.20 " = 49 " 27.2—27.6° C.
- 78.75—39.5 " = 3.47 " —4.31 " = 44 " 26.75—27.2° C.
- 39.5—19.75 " = 3.0 " —3.47 " = 47 " 26.75—26.6—26.8° C.
- 19.75—9.75 " = 2.15 " —3.0 " = 45 " 27—26.8° C.

\* Had to re-arrange, and open finger windows.



Normal doubling period:—

36—72  $\mu$  = 3.40 P.M.—4.26 P.M. = 46 minutes at 26.6—27.1° C.

*Doubling Periods.*

This is a good opportunity for explaining more in detail what is meant by the above term.

If this long and symmetrical curve 83 is carefully examined, it will be found that, over a great part of its course, *any chosen short length* of the filament *doubles* its length in approximately equal times.

Thus it took forty-five minutes to grow from  $9\ \mu$  to  $18\ \mu$ , and the same from  $18\ \mu$  to  $36\ \mu$ , and so on. But it is practically the same whatever the length we select, *provided the conditions are constant*, as will be seen by comparing the curves.

I term the period required for doubling the length the "*doubling period*," and the comparison of a large number of curves shows that the nearer we approximate to absolute constancy of the conditions the clearer the truth of the following statement, which amounts to a law, is evident.

*Whatever the length of the filament taken, that length is doubled in equal times when the conditions are constant.*

For the present it is only necessary to clearly apprehend the meaning of the above term, and the general truth enunciated, so that the reader can understand the figures attached to the tables. Further particulars will come out as we proceed.

The following experiment at  $30^{\circ}$  was carried out on July 18. The spores, in stiff weak gelatine, were sown and put in the Sachs' box at 9.30 A.M.

The temperatures ran as follows:—

| Time.        | Temp.<br>°C.                          |
|--------------|---------------------------------------|
| 9.30 A.M. =  | 30.5                                  |
| 10.0    " =  | 31.5                                  |
| 10.20   " =  | 30.6                                  |
| 10.30   " =  | 30.2                                  |
| 11.0     " = | 30.25                                 |
| 11.20   " =  | 30.4                                  |
| 11.35   " =  | 30.5                                  |
| 12.0 noon =  | 31.0                                  |
| 12.15 P.M. = | 31.1                                  |
| 12.30   " =  | 30.5                                  |
| 1.30    " =  | 30.0                                  |
| 1.45    " =  | 24.75 (due to opening box to arrange) |
| 1.47   " =   | 27.0       "       "       "          |
| 1.51   " =   | 29.0       "       "       "          |
| 2.0     " =  | 30.0                                  |
| 2.30   " =   | 31.0                                  |
| 3.15   " =   | 31.0                                  |

On now examining I found the germinated bacilli had grown into curiously contorted and stunted colonies, and there stopped. No further growth could be observed, although I slowly lowered the temperature as follows :—

| Time.       | Temp.<br>° C. |
|-------------|---------------|
| 3.45 P.M. = | 29.5          |
| 4.25 „ =    | 25.0          |
| 4.35 „ =    | 24.75         |
| 5.0 „ =     | 25.4          |
| 5.20 „ =    | 25.75         |
| 6.15 „ =    | 24.75         |
| 7.0 „ =     | 24.0          |

A rodlet fixed on the scale and measured showed no departure at 7 P.M. from its original length, 9  $\mu$ , at 5 P.M. Growth was impossible, apparently, under these conditions.

I presume that what occurred was, germination was effected, but, at the temperature 30—31° C., the delicate young rodlets could not assimilate food-materials from this weak gelatine.

Later considerations, based on further experience, suggests that the variations of temperature may have combined to inhibit the growth also.

On reviewing these curves of growth at various temperatures, kept approximately constant in each case, in the weak gelatine, we find the following facts come out :—

1. As was to be expected, the doubling periods are longer at lower temperatures than at higher ones.

2. There are indications of a temperature near 25° C. being more favourable than any others, i.e., an *optimum* temperature, as contrasted with less favourable lower temperatures nearer a *minimum*, or higher ones nearer a *maximum*.

3. Some facts point to the necessity for distinguishing the germinal phase of growth from the more independent growth which occurs after the rodlet is entirely free of the spore and its reserves.

4. The doubling period is evidently affected not only by the mean temperature employed, but by variations above and below this mean ; and this in two ways : (1) according to the *rapidity* of these variations, and (2) according to their *range*.

It is hardly worth while making any more general statements until we have examined other cases, so I now pass to cultures in broth.

#### B. Cultures in Broth.

On July 23 a broth-culture of spores was started in the Sachs' box, darkened, at 1.30. The temperature was 17.2°, and remained steadily

at 17° C. for some time, slowly falling during the afternoon to 16.75° by 4.30. On then opening to search the drop, the temperature rose to 17.4° C., falling to 17° at 5 P.M., and 16.8 at 5.15. Germination had begun at 5 P.M. The measurements began at 5.30 as follows, and were carried through the night.

| Time.      | Length. | Interval. | Growth. | Rate.   | Temp. |
|------------|---------|-----------|---------|---------|-------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 5.30 P.M.  | 4.0     | —         | —       | —       | 16.75 |
| 6.0 "      | 6.75    | 30        | 2.75    | 0.09    | 16.75 |
| 6.30 "     | 9.0     | 30        | 2.25    | 0.07    | 16.7  |
| 7.30 "     | 14.0    | 60        | 5.0     | 0.08    | 16.5  |
| 8.30 "     | 24.75   | 60        | 10.75   | 0.18    | 16.5  |
| 9.20 "     | 35.0    | 50        | 10.25   | 0.2     | 16.25 |
| 10.10 "    | 51.75   | 50        | 16.75   | 0.33    | 16.3  |
| 10.45 "    | 67.5    | 35        | 15.75   | 0.45    | 16.3  |
| *11.32 "   | 99.0    | 47        | 31.5    | 0.67    | 16.0  |
| 12.25 A.M. | 148.5   | 53        | 47.5    | 0.9     | 16.25 |
| †12.57 "   | 191.25  | 32        | 42.75   | 1.33    | 16.25 |
| 1.27 "     | 238.5   | 30        | 47.25   | 1.57    | 16.5  |
| 2.20 "     | 337.5   | 53        | 99.0    | —       | 16.5  |

Curve 84, p. 402.

The doubling periods were as follows :—

1. 4— 8  $\mu$  = 48 minutes at 16.75° C.
2. 8— 16 " = 82 " 16.75—16.5° C.
3. 16— 32 " = 86 " 16.5—16.25° C.
4. 32— 64 " = 92 " 16.25—16.3° C.
5. 64—128 " = 86 " 16.3—16—16.25° C.
6. 128—256 " = 92 " 16.25—16.5° C.

Calculated back :—

$$\begin{aligned}
 337.5 - 168.75 \mu &= 100 \text{ minutes.} \\
 168.75 - 84.5 &= 86 \text{ " } \\
 84.5 - 42.25 &= 90 \text{ " } \\
 42.0 - 21.0 &= 94 \text{ " } \\
 21.0 - 10.5 &= 78 \text{ " } \\
 10.5 - 5.25 &= 70 \text{ " }
 \end{aligned}$$

Normal doubling periods :—

$$\begin{aligned}
 6.75 - 13.5 \mu &= 6.0 \text{ P.M.} - 7.20 \text{ P.M.} = 80 \text{ mins. at } 16.75 - 16.5^\circ \text{ C.} \\
 13.5 - 27.0 &= 7.20 \text{ " } - 8.40 \text{ " } = 80 \text{ " } 16.5^\circ \text{ C.} \\
 27.0 - 54.0 &= 8.40 \text{ " } - 10.17 \text{ " } = 97 \text{ " } 16.5 - 16.25 - 16.3^\circ \text{ C.} \\
 54.0 - 108.0 &= 10.17 \text{ " } - 11.42 \text{ " } = 85 \text{ " } 16.3 - 16.0^\circ \text{ C.} \\
 108.0 - 216.0 &= 11.42 \text{ " } - 1.13 \text{ A.M.} = 91 \text{ " } 16.0 - 16.5^\circ \text{ C.}
 \end{aligned}$$

\* Rose at once to 17.1° C. on opening side windows, falling again to 16.5° C. in five minutes.

† Up to 16.75° C. on opening side window, down to 16.5° C. in five minutes.

As regards the discrepancy between the first doubling period and the rest, I have little doubt it is explicable with reference to the process of germination. It must be noted that the initial length ( $4\ \mu$ ) is not that of a free rodlet working up its food-materials from the broth, but of rodlet *plus* spore, and presumably still utilising unexpended stores from the spore. To make the comparisons fair, therefore, we ought to neglect this first period.

On July 21 and 22 the following observations were started with cultures in fresh broth:—

Spores sown 8 P.M. were put at once into Sachs' box at  $19.5^{\circ}\text{C}$ ., falling to  $18.75^{\circ}$  at 9.30 P.M.

On July 22 the growths, followed for 1 hour and 10 minutes, gave the doubling period:—

$$99-198\ \mu \text{ in 65 minutes at } 17-16.75-17^{\circ}\text{C.}$$

Or, calculating back:—

$$225-112.5\ \mu = 8.22\ \text{A.M.}-9.10\ \text{A.M.} = 48\ \text{minutes at } 16.75-17^{\circ}\text{C.}$$

Normal doubling period:—

$$112-224\ \mu = 8.22\ \text{A.M.}-9.10\ \text{A.M.} = 48\ \text{minutes at } 16.75-17^{\circ}\text{C.}$$

This culture formed spores by 9 A.M., July 25, having been at  $16-19^{\circ}$  throughout.

On July 22 a culture started in broth at  $17^{\circ}\text{C}$ . at 8 A.M., had germinated out about noon, and the measurements were made from 4.55 P.M. to 8.28 P.M. Sachs' box, &c., as before.

Here the doubling periods were as follows:—

1.  $38.25-76.5\ \mu = 68\ \text{minutes at } 19-18.2^{\circ}\text{C.}$
2.  $76.5-153\ \mu = 72\ \text{,, } 18.2-18^{\circ}\text{C.}$
3.  $153-306\ \mu = 73\ \text{,, } 18-18.2^{\circ}\text{C.}$

Normal doubling period:—

$$56.5-113\ \mu = 5.30\ \text{P.M.}-6.41\ \text{P.M.} = 71\ \text{minutes at } 18.5-18^{\circ}\text{C.}$$

$$113-226\ \mu = 6.41\ \text{,, } -7.54\ \text{,,} = 73\ \text{,, } 18-^{\circ}\text{C.}$$

This culture had formed spores at 9 A.M. on the 25th, having been at  $16-19^{\circ}$  in dark Sachs' box the whole time.

If we calculate at other points in the curve, we get doubling as follows:—

$$112.5-225\ \mu = 6.40\ \text{P.M.}-7.54\ \text{P.M.} = 74\ \text{minutes at } 19^{\circ}\text{C.}$$

$$50.5-101\ \mu = 5.20\ \text{,, } -6.29\ \text{,,} = 69\ \text{,, } 18.5-18^{\circ}\text{C.}$$

On July 24 I sowed spores in broth at 2.30 A.M., and put at once into the Sachs' box at  $17^{\circ}\text{C}$ . The temperature remained constant through the early morning, and measurements were made as follows:—

| Time.      | Length. | Interval. | Growth. | Rate.   | Temp.         |
|------------|---------|-----------|---------|---------|---------------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 8.53 A.M.  | 11.25   | —         | —       | —       | 17.1          |
| 9.40 "     | 15.75   | 47        | 4.50    | 0.09    | 17.0          |
| 10.20 "    | 24.75   | 40        | 9.0     | 0.22    | 16.9          |
| 10.50 "    | 31.5    | 30        | 6.75    | 0.22    | 17.0          |
| 11.30 "    | 41.5    | 40        | 10.0    | 0.25    | 17.0          |
| 12 noon    | 55.0    | 30        | 13.5    | 0.45    | 17.1          |
| 12.20 P.M. | 65.0    | 20        | 10.0    | 0.5     | 17.4          |
| 1.12 "     | 101.25  | 52        | 36.25   | 0.7     | 18.0          |
| 1.34 "     | 123.75  | 22        | 22.5    | 1.0     | 18.0          |
| 2.0 "      | 156.0   | 26        | 32.25   | 1.24    | 18.0          |
| 2.34 "     | 211.5   | 34        | 55.5    | 1.6     | 18.1          |
| 2.45 "     | 234.0   | 11        | 22.5    | 2.0     | 18.2          |
| 3.0 "      | 270.0   | 15        | 36.0    | 2.4     | 18.25         |

Curve 87, p. 402.

The doubling periods are here as follows:—

1. 11.25—22.5  $\mu$  = 79 minutes at 17.1—16.9° C.
2. 22.5—45 " = 86 " 16.9—17° C.
3. 45—90 " = 79 " 17—18° C.
4. 90—180 " = 78 " 18° C.

Calculated back:—

- 270—135  $\mu$  = 1.42 P.M.—3.0 P.M. = 78 minutes at 18—18.25° C.  
 135—67.5 " = 12.22 " — 1.42 " = 80 " 17.4—18° C.  
 67.5—33.75 " = 11.0 A.M.—12.22 P.M. = 82 " 17—17.4° C.  
 33.75—16.5 " = 9.46 " —11.0 A.M. = 74 " 17—16.9—17° C.

Normal doubling periods:—

- 13.5—27  $\mu$  = 9.20 A.M.—10.30 A.M. = 70 minutes at 17—16.9° C.  
 27—54 " = 10.30 " —11.58 " = 88 " 16.9—17° C.  
 54—108 " = 11.58 " — 1.19 P.M. = 79 " 17—18° C.  
 108—216 " = 1.19 P.M.— 2.37 " = 78 " 18—18.2° C.

In the same culture I selected another filament at 4.45 P.M., July 24, and measured from 4.46 P.M. to 6.24 P.M.

The doubling periods were here as follows:—

- 85.5—171.0  $\mu$  = 77 minutes at 18.75—19° C.

Calculated back:—

- 220.5—110.25  $\mu$  = 5.17 P.M.—6.24 P.M. = 67 minutes at 18.75—19.5° C.

Spores were beginning to form in some filaments at 8.0 P.M. on July 26, but, even on 28th, few, sparse, and small spores, much poorer than those of 25th July.

On July 25 spores in broth were sown at 9.30 A.M., and at once put into Sachs' box, dark, at 19° C. The temperature rose slowly to 19.5° at 10.15, and to 20.25° at 2 P.M.; thence it remained at 20.25—20.75, a rodlet being selected at 3.35 P.M. and kept under observation till 6.20 P.M.

Doubling periods:—

1. 20·25—40·5  $\mu$  in 49 minutes at 20·75—20·25° C.
2. 40·5 — 81    „ „ 56        „    20·25—20·6° C.
3. 81    —162    „ „ 63        „    20·6—20·5° C.

Calculated back:—

- 157 —78·5  $\mu$  = 5.18 P.M.—6.20 P.M. = 62 minutes at 20·6—20·5° C.  
 78·5—39·5 „ = 4.22 „ —5.18 „ = 56 „    20·25—20·6° C.

Normal doubling periods:—

- 33·75—57·5  $\mu$  = 4.0 P.M.—5.6 P.M. = 66 minutes at 20.5—20·25—20·6° C.  
 67·5 —135·0 „ = 5.6 „ —6.6 „ = 60 „    20·6—20·5° C.

This 25th July culture had formed splendid spores by 10.45 P.M. on the 27th, whence the whole life cycle took about 60 hours at 20—23° C., having been in the dark under a bell-jar since 26th, 9 A.M.

On August 4 a spore culture in broth was started at 7 A.M. at 21° C., rising very slowly, the variations were:—

| Time.    | Temp.<br>°C. |
|----------|--------------|
| 7.0 A.M. | = 21·0       |
| 9.15 „   | = 22·25      |
| 9.30 „   | = 22·5       |
| 9.45 „   | = 22·5       |
| 9.55 „   | = 23·2       |
| 10.30 „  | = 23·5       |
| 10.55 „  | = 23·7       |
| 11.22 „  | = 23·7       |

Germination began about 9.15, as I saw a spore swollen and elongating then; at 9.45 several were seen from 4  $\mu$  to 6  $\mu$  long, and at 10.30 one was selected for measurement 10  $\mu$  long, as follows:—

| Time.      | Length. | Interval. | Growth. | Rate.   | Temp. |
|------------|---------|-----------|---------|---------|-------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | °C.   |
| 10.30 A.M. | 10·0    | —         | —       | —       | 23·5  |
| 10.55 „    | 20·25   | 25        | 10·25   | 0·4     | 23·7  |
| 11.25 „    | 31·5    | 30        | 11·25   | 0·4     | 23·7  |
| 11.42 „    | 40·5    | 17        | 9·0     | 0·5     | 23·7  |
| 12.0 noon  | 56·25   | 18        | 15·75   | 0·9     | 23·75 |
| 12.20 P.M. | 76·5    | 20        | 20·25   | 1·0     | 24·0  |
| 12.40 „    | 105·75  | 20        | 29·25   | 1·45    | 24·5  |
| 12.50 „    | 126·0   | 10        | 20·25   | 2·0     | 24·6  |
| 1.0 „      | 153·0   | 10        | 27·0    | 2·7     | 24·75 |
| 1.25 „     | 229·5   | 25        | 76·5    | 3·0     | 25·0  |
| 2.25 „     | 652·5   | 60        | 423·0   | 7·0     | 25·0  |

Doubling periods :—

1. 10—20  $\mu$  = 10.30 A.M.—10.54 A.M. = 24 minutes at 23.5—23.7° C.
2. 20—40 „ = 10.54 „ —11.41 „ = 47 „ 23.7° C.
3. 40—80 „ = 11.41 „ —12.22 P.M. = 41 „ 23.7—24° C.
4. 80—160 „ = 12.22 P.M.—1.3 „ = 41 „ 24—24.75° C.
5. 160—320 „ = 1.3 „ —1.40 „ = 37 „ 24.75—25° C.
6. 320—640 „ = 1.40 „ —2.23 „ = 43 „ 25° C.

Calculated back :—

- 459—229.5  $\mu$  = 1.25 P.M.—2.5 P.M. = 40 minutes at 25° C.
- 229.5—114.75 „ = 12.45 „ —1.25 „ = 40 „ 24.5—25° C.
- 114.75—57.5 „ = 11.59 A.M.—12.45 „ = 46 „ 23.75—24.5° C.
- 57.5—28.75 „ = 11.19 „ —11.59 A.M. = 40 „ 23.7—23.75° C.
- 28.75—14.5 „ = 10.40 „ —11.19 „ = 39 „ 23.7° C.

Normal doubling periods :—

- 22—44  $\mu$  = 10.58 A.M.—11.46 A.M. = 48 minutes at 23.7° C.
- 44—88 „ = 11.46 „ —12.28 P.M. = 42 „ 23.7—24° C.
- 88—176 „ = 12.28 P.M.—1.8 „ = 40 „ 24—25° C.
- 176—352 „ = 1.8 „ —1.42 „ = 36 „ 25° C.
- \*352—704 „ = 1.42 „ —2.32 „ = 50 „ 25° C.

On August 6 a broth-drop of spores was started in the Sachs' box at 10.15 A.M. Temperatures ran as follows :—

| Time.      | Temp.<br>° C. |
|------------|---------------|
| 10.15 A.M. | = 22.0        |
| 10.50 „    | = 24.5        |
| 11.5 „     | = 25.0        |
| 11.30 „    | = 24.9        |
| 11.45 „    | = 24.5        |
| 12.0 noon  | = 24.5        |
| 2.0 P.M.   | = 26.0        |
| 2.30 „     | = 26.0        |
| 2.50 „     | = 26.0        |

The measurements were now started on a filament 47.25  $\mu$  long, and followed till 4.8 P.M. ; the temperature remained at 26—25.75°.

Doubling periods :—

1. 47.25—94.5  $\mu$  = 2.50 P.M.—3.20 P.M. = 30 minutes at 26—25.9° C.
2. 94.5—189 „ = 3.20 „ —3.58 „ = 38 „ 25.9—25.75° C.

Or, calculated back :—

- 225—112.5  $\mu$  = 3.30 P.M.—4.8 P.M. = 38 minutes at 25.9—25.75° C.
- 112.5—56.25 „ = 2.57 „ —3.30 „ = 33 „ 26—25.9° C.

Normal doubling period :—

- 47.25—94.5  $\mu$  = 2.50 P.M.—3.20 P.M. = 30 minutes at 26—25.9° C.

\* Partly calculated ; would probably have been less.

Spores had begun to round off at 7 A.M. on August 7, i.e., life cycle in less than 24 hours.

On August 3 spores were sown in a broth-drop at 11.30 A.M., and put to germinate in the incubator at 23° C.; they did this rapidly and normally about 3.30, and at 3.50 the culture was put into the Sachs' box. During the accommodation period the temperature fell from 28·6° at 3.50 to 27·25° at 4.40, when a germinating rodlet was selected for measurement, as follows:—

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 4.40 P.M. | 6·0     | —         | —       | —       | 27·25 |
| 5.0 "     | 8·0     | 20        | 2·0     | 0·1     | 27·0  |
| 5.20 "    | 10·0    | 20        | 2·0     | 0·1     | 26·75 |
| 5.40 "    | 14·5    | 20        | 4·5     | 0·2     | 26·5  |
| 6.0 "     | 22·5    | 20        | 8·0     | 0·4     | 26·5  |
| 6.20 "    | 33·75   | 20        | 11·25   | 0·5     | 26·4  |
| 6.40 "    | 47·25   | 20        | 13·5    | 0·67    | 26·5  |
| 7.0 "     | 68·5    | 20        | 21·25   | 1·0     | 26·6  |
| 7.20 "    | 103·5   | 20        | 35·0    | 1·7     | 26·4  |

Normal doubling period:—

8—16  $\mu$  = 5 P.M.—5.44 P.M. = 44 minutes at 27·0—26·5° C.

but as this was the germinating phase, take—

16—32  $\mu$  = 5.44 P.M.—6.18 P.M. = 34 minutes at 26·5—26·4° C.

Doubling periods:—

1. 6—12  $\mu$  = 4.40 P.M.—5.29 P.M. = 49 minutes at 27·25—26·7° C.
2. 12—24 " = 5.29 " —6.3 " = 34 " 26·7 —26·5° C.
3. 24—48 " = 6.3 " —6.41 " = 38 " 26·5 —26·4—26·5° C.
4. 48—96 " = 6.41 " —7.16 " = 35 " 26·5 —26·6° C.

Calculated back:—

103·5 —51·75  $\mu$  = 6.42 P.M.—7.20 P.M. = 38 minutes at 26·5—26·6—26·4° C.  
 51·75—26·0 " = 6.6 " —6.42 " = 36 " 26·5—26·4—26·5° C.  
 26·0 —13·0 " = 5.33 " —6.6 " = 33 " 26·6—26·4° C.

Another filament, selected at 8.40, gave the following:—

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 8.43 P.M. | 100·75  | —         | —       | —       | 28·25 |
| 8.56 "    | 121·5   | 13        | 20·75   | 1·6     | 28·25 |
| 9.15 "    | 173·25  | 19        | 51·75   | 2·7     | 27·5  |
| 9.32 "    | 216·0   | 17        | 42·75   | 2·5     | 27·0  |
| 9.45 "    | 252·0   | 13        | 36·0    | 2·8     | 27·0  |

Doubling period :—

100·75—201·5  $\mu$  = 8.43 P.M.—9.26 P.M. = 43 minutes at 28·25—27° C.

Or, reckoned back :—

252—126  $\mu$  = 8.58 P.M.—9.45 P.M. = 47 minutes at 28·25—27° C.

On August 2 a broth-culture of spores was started at 21·25° in dark at 9.40 A.M. Germination was very rapid at 21·25—21·5°, the temperature being nearly constant, and at 1.45 there were rods from 45· $\mu$  to 70  $\mu$  and longer.

I then rapidly raised the temperature from 21·6° at 1.45 as follows :—

| Time.    | Temp.<br>° C. |
|----------|---------------|
| 2.0 P.M. | = 25·0        |
| 2.2 "    | = 27·0        |
| 2.4 "    | = 28·0        |
| 2.5 "    | = 28·5        |
| 2.6 "    | = 29·0        |
| 2·8 "    | = 29·5        |
| 2.10 "   | = 29·7        |

and selected a rod, in two segments at an angle of 75° or so, for measurement.

The variations of temperature between 2.10 and 2.40 should be noticed. They were—

| Time.     | Temp.<br>° C. |                               |
|-----------|---------------|-------------------------------|
| 2.10 P.M. | = 29·7        |                               |
| 2.12 "    | = 30·0        | } Fall due to opening the box |
| 2.13 "    | = 29·0        |                               |
| 2.14 "    | = 29·3        |                               |
| 2.15 "    | = 29·5        |                               |
| 2.16 "    | = 29·7        |                               |
| 2.18 "    | = 29·9        |                               |
| 2.20 "    | = 30·0        |                               |
| 2.25 "    | = 29·9        |                               |
| 2.30 "    | = 29·9        |                               |
| 2.40 "    | = 30·0        |                               |

From thence onwards the temperature changed very slowly: equilibrium was attained.

#### Segment A.

This filament was followed from 2.10 P.M. to 3.10 P.M. and had then

to be abandoned, owing to the pronounced curvatures often setting in at these higher temperatures.

Doubling period :—

18—36  $\mu$  = 2.10 P.M.—2.53 P.M. = 43 minutes at 29.7—30—29.9—30° C.

Calculated back :—

51.75—25.75  $\mu$  = 2.32 P.M.—3.10 P.M. = 38 minutes at 29.9—30—29.9° C.

Normal doubling period :—

24.75—49.5  $\mu$  = 2.30 P.M.—3.9 P.M. = 39 minutes at 29.9—30—29.9° C.

#### *Segment B.*

Was followed from 2.10 P.M. to 3.10 P.M., and then abandoned.

Doubling period :—

18—36  $\mu$  = 2.10 P.M.—3.0 P.M. = 50 minutes at 29.7—30—29.9—30° C.

Calculated back :—

45—22.5  $\mu$  = 2.30 P.M.—3.10 P.M. = 40 minutes at 29.9—30—29.9° C.

Normal doubling period :—

22.5—45  $\mu$  = 2.30 P.M.—3.10 P.M. = 40 minutes at 29.9—30—29.9° C.

#### *Total.*

Doubling period :—

36—72  $\mu$  = 2.10 P.M.—2.57 P.M. = 47 minutes at 29.7—30—29.9—30° C.

Or, calculated back :—

96—48  $\mu$  = 2.31 P.M.—3.10 P.M. = 39 minutes at 29.9—30—29.9° C.

Another segment in the same culture was chosen at 3.24 P.M. and followed till 4.10 P.M.

Doubling period :—

118—236  $\mu$  = 3.24 P.M.—4.3 P.M. = 39 minutes at 29—29.75—28.75° C.

Or, calculating back :—

260—130  $\mu$  = 3.28 P.M.—4.10 P.M. = 42 minutes at 29—29.75—28.75° C.

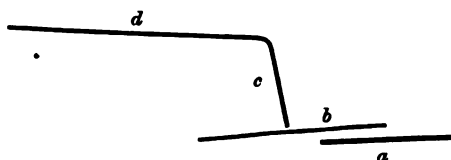
On August 7 a broth-drop of spores was put into the incubator at 23° C. at 7.30 A.M., and left to germinate. At 10.40 there were rods 36  $\mu$  to 45  $\mu$  long, and the culture was put into Sachs' box at 31° C.; during accommodation the temperatures ran—

| Time.        | Temp.<br>° C. |
|--------------|---------------|
| 10.49 A.M. = | 29.5          |
| 11.0    „ =  | 31.0          |
| 11.15   „ =  | 31.0          |

At 11.30 a rod bent thus \\_ was selected and measured as follows:—The total measurement is given, and the behaviour of the segments into which (as commonly at high temperatures) the filament broke up are referred to later.

This filament was an interesting one in several respects, for its segments broke off so conveniently, or remained joined at such convenient angles, that I was able to measure them much more accurately than sometimes occurs.

When first observed, the filament consisted of two segments joined at an angle of about \\_. At 12 noon the horizontal leg had broken into two rods, the ends of which were already sliding one over the other, thus \\_, and by 12.20 the other leg had bent into two segments, which remained joined thus—



Consequently, I was able to measure all four segments separately, and, for convenience, I term them A, B, breaking up into *a*, *b*, and *c*, *d*, as shown in the diagram. The following are their measurements; the intervals occupied in these were too small to record.

First, I take the whole of the leg B, A and the whole of the leg C, D, and give their measurements separately, then their separate segments from the time they became distinct.

#### Total Length.

(From 11.30 A.M. to 1 P.M.)

Doubling periods:—

1. 42.75—85.5  $\mu$  = 11.30 A.M.—12.0 noon = 30 minutes at 31.1—30.75° C.
2. 85.5—171.0 „ = 12.0 noon—12.36 P.M. = 36 „ 30.75—30.8—31° C.

Or, calculated back:—

- 270—135.0  $\mu$  = 12.25 P.M.—1.0 P.M. = 35 minutes at 31—31.25° C.
- 135—67.5 „ = 11.52 A.M.—12.25 „ = 33 „ 31—30.75—31° C.

Normal doubling period:—

- 42.75—85.5  $\mu$  = 11.30 A.M.—12.0 noon = 30 minutes at 31.1—30.75° C.

#### Segment A.

(Comprising, after 12 noon, *b* and *a*.)

Doubling periods:—

1. 22.5—45  $\mu$  = 11.30 A.M.—12.0 noon = 30 minutes at 31.1—30.75° C.
2. 45.0—90 „ = 12.0 noon—12.28 P.M. = 38 „ 30.75—31° C.

Or, reckoned back :—

$$\begin{aligned} 143 \cdot 75 - 72 \mu &= 12.27 \text{ P.M.} - 1.0 \text{ P.M.} = 33 \text{ minutes at } 31 - 31 \cdot 25^\circ \text{ C.} \\ 72 \cdot 0 - 36 \mu &= 11.52 \text{ A.M.} - 12.27 \text{ „} = 35 \text{ „} \quad 31 - 30 \cdot 75 - 31^\circ \text{ C.} \end{aligned}$$

*Segment B.*

Doubling periods :—

$$\begin{aligned} 20 \cdot 25 - 40 \cdot 5 \mu &= 11.30 \text{ A.M.} - 12.0 \text{ noon} = 30 \text{ minutes at } 31 \cdot 1 - 30 \cdot 75^\circ \text{ C.} \\ 40 \cdot 5 - 81 \cdot 0 \mu &= 12.0 \text{ noon} - 12.35 \text{ P.M.} = 35 \text{ „} \quad 30 \cdot 75 - 31^\circ \text{ C.} \end{aligned}$$

Reckoned back :—

$$\begin{aligned} 128 \cdot 25 - 64 \mu &= 12.23 \text{ P.M.} - 1.0 \text{ P.M.} = 37 \text{ minutes at } 31 - 31 \cdot 25^\circ \text{ C.} \\ 64 \cdot 0 - 32 \mu &= 11.53 \text{ A.M.} - 12.35 \text{ „} = 42 \text{ „} \quad 31 - 30 \cdot 75 - 31^\circ \text{ C.} \end{aligned}$$

*Segment a.*

(From 12 noon to 1 P.M.)

Doubling period :—

$$22 \cdot 5 - 45 \mu = 12.0 \text{ noon} - 12.35 \text{ P.M.} = 35 \text{ minutes at } 30 \cdot 75 - 31^\circ \text{ C.}$$

Or, reckoning back :—

$$73 \cdot 25 - 36 \cdot 5 \mu = 12.27 \text{ P.M.} - 1.0 \text{ P.M.} = 33 \text{ minutes at } 31 - 31 \cdot 25^\circ \text{ C.}$$

*Segment b.*

(From 12 noon to 1 P.M.)

Doubling period :—

$$22 \cdot 5 - 45 \mu = 12 \text{ noon} - 12.41 \text{ P.M.} = 41 \text{ minutes at } 30 \cdot 75 - 31^\circ \text{ C.}$$

Or calculated back :—

$$67 \cdot 5 - 33 \cdot 75 \mu = 12.26 \text{ P.M.} - 1.0 \text{ P.M.} = 34 \text{ minutes at } 31 - 31 \cdot 25^\circ \text{ C.}$$

*Segment c.*

(From 12.30 to 1 P.M.)

This segment did not reach the doubling point, but, if the curve is prolonged to 1.3 P.M., we get it, which gives—

Part calculated :—

$$22 \cdot 5 - 45 \mu = 12.30 \text{ P.M.} - 1.3 \text{ P.M.} = 33 \text{ minutes at } 31 - 31 \cdot 25^\circ \text{ C.}$$

*Segment d.*

(From 12.30 to 1 P.M.)

Here, again, the doubling period was not reached, but prolongation of the curve to 1.10 P.M. gives—

Part calculated :—

$$49 \cdot 5 - 99 \mu = 12.30 \text{ P.M.} - 1.10 \text{ P.M.} = 40 \text{ minutes at } 31 - 31 \cdot 25^\circ$$

At 1.40 another filament in this drop was selected and measured as follows, from 1.48 P.M. to 3.10 P.M. :—

Doubling period :—

58·5—117  $\mu$  = 1.48 P.M.—2.42 P.M. = 54 minutes at 31·5—31·75° C.

Calculated back :—

170·5—85·25  $\mu$  = 2.16 P.M.—3.10 P.M. = 66 minutes at 31·5—32° C.

A review of these broth-cultures bears out the general truth of the previous statements, and justifies the following additional conclusions :—

1. The growth in broth is more rapid than in the weak gelatine, and the doubling periods, especially at lower temperatures, are correspondingly shorter.

2. A curious technical difficulty arises, and often gives great trouble where broth is used (or when, at high temperatures, the gelatine liquefies). On opening the side windows to rearrange the object for measurement a slight draught is produced, and a cooling of the cover-slip ensues; this causes condensation of the vapour in the cell, and, if a drop catches the edge of the hanging drop, currents are produced, and the filament, if short and floating free, may move out of the field. Slight and slow movements may be followed, and all go on as before, but rapid ones are fatal to the purpose.

3. Several of the cultures suggest that a period of exhaustion gradually supervenes as the culture ages; it remains to be seen how far this is due to one or other of the following possible causes: (1) mere using up of the food-materials; (2) fouling of the drop by excreta; (3) or by CO<sub>2</sub> accumulating as the oxygen is consumed in respiration.

#### *C. Cultures in Broth + 1 per cent. Gelatine.*

On August 1 spores were sown as before, broth + 1 per cent. gelatine, and in at 9.30 A.M., at temperatures as follows :—

| Time.     | Temp.<br>° C. |
|-----------|---------------|
| 9.30 A.M. | = 22·0        |
| 10.25 „   | = 22·0        |
| 11.00 „   | = 22·0        |
| 11.30 „   | = 22·2        |
| 11.57 „   | = 22·0        |
| 12.10 „   | = 22·2        |
| 12.40 „   | = 21·7        |
| 2.00 P.M. | = 20·5        |
| 2.20 „    | = 20·4        |
| 2.28 „    | = 21·1        |

Germination had set in normally, and rodlets up to  $50\ \mu$  were found. I selected one  $37\ \mu$  long, divided into two segments of  $18\ \mu$  and  $19\ \mu$  respectively, and measured as follows, taking each of its two segments separately; they were joined at an angle of about  $165^\circ$ :—

*Segment A.*

Doubling periods:—

1.  $19-38\ \mu = 2.28\ \text{P.M.}-3.24\ \text{P.M.} = 56\ \text{minutes at } 21.1-21-21.5^\circ\ \text{C.}$
2.  $38-76\ \mu = 3.24\ \mu - 4.35\ \mu = 71\ \mu \quad 21.5-22-22.25-21.8^\circ\ \text{C.}$

Or, calculated back:—

- $103.5-51.25\ \mu = 3.53\ \text{P.M.}-5.0\ \text{P.M.} = 67\ \text{mins. at } 22-22.25-21.8-22.7^\circ\ \text{C.}$
- $51.25-25.5\ \mu = 2.55\ \mu - 3.53\ \mu = 58\ \mu \quad 21.2-22^\circ\ \text{C.}$

Normal doubling period:—

$$27-54\ \mu = 60\ \text{minutes at } 21.5-22.1^\circ\ \text{C.}$$

*Segment B.*

Doubling periods:—

1.  $18-36\ \mu = 2.28\ \text{P.M.}-3.26\ \text{P.M.} = 58\ \text{minutes at } 21.1-21-21.5^\circ\ \text{C.}$
2.  $36-72\ \mu = 3.26\ \mu - 4.30\ \mu = 64\ \mu \quad 21.5-22-22.25^\circ\ \text{C.}$

Normal doubling period:—

$$27-54\ \mu = 3\ \text{P.M.}-4\ \text{P.M.} = 60\ \text{minutes at } 21.25-22.1^\circ\ \text{C.}$$

*Total.*

Doubling periods:—

1.  $37-74\ \mu = 2.28\ \text{P.M.}-3.24\ \text{P.M.} = 56\ \text{minutes at } 21.1-21-21.5^\circ\ \text{C.}$
2.  $74-148\ \mu = 3.24\ \mu - 4.33\ \mu = 69\ \mu \quad 21.5-22.25-22^\circ\ \text{C.}$

Or, calculating back:—

- $207.0-103.5\ \mu = 3.55\ \text{P.M.}-5.0\ \text{P.M.} = 65\ \text{minutes at } 22-22.25-21.7^\circ\ \text{C.}$
- $103.5-51.25\ \mu = 2.55\ \mu - 3.55\ \mu = 60\ \mu \quad 21.1-22^\circ\ \text{C.}$

On July 30 spores were sown in broth + 1 per cent. gelatine, and put in at 8 A.M. Germination was very rapid at 23.75 (8 A.M.), 23.5 (9.20), 24 (10 A.M.), and at 10.45 a rod  $9\ \mu$  long was selected.

Doubling periods:—

1.  $9-18\ \mu = 10.45\ \text{A.M.}-11.30\ \text{A.M.} = 45\ \text{minutes at } 24.5^\circ\ \text{C.}$
2.  $18-36\ \mu = 11.30\ \mu - 12.12\ \text{P.M.} = 42\ \mu \quad 24.6^\circ\ \text{C.}$
3.  $36-72\ \mu = 12.12\ \text{P.M.}-12.56\ \mu = 44\ \mu \quad 24.5-24.8^\circ\ \text{C.}$

Or, calculated back:—

- $76.5-38.25\ \mu = 12.17\ \text{P.M.}-1.0\ \text{P.M.} = 43\ \text{minutes at } 24.5-24.8^\circ\ \text{C.}$
- $38.5-19.0\ \mu = 11.33\ \text{A.M.}-12.17\ \mu = 44\ \mu \quad 25.0^\circ\ \text{C.}$
- $19.0-9.5\ \mu = 10.47\ \mu - 11.33\ \text{A.M.} = 44\ \mu \quad 25.0^\circ\ \text{C.}$

Normal doubling periods :—

14—28  $\mu$  = 11.15 A.M.—11.53 A.M. = 38 minutes at 24.5° C.  
 28—56 „ = 11.53 „ —12.39 P.M. = 46 „ 24.5—24.8° C.

The rapid germination indicates the neighbourhood of optimum temperature.

This filament was now lost, and another taken as follows :—

Doubling period :—

1. 67.5—135  $\mu$  = 2 P.M.—2.50 P.M. = 50 minutes at 24.5—24.75—24.6° C.

Or, calculating back :—

238—119  $\mu$  = 2.41 P.M.—3.25 P.M. = 44 minutes at 24.75—24.5° C.

On July 28 spores in broth + 1 per cent. gelatine were sown and put into Sach's box at 23.5° C. at 8 A.M. The temperature was slowly raised to 26° C. as follows :—

| Time.      | Temp.<br>° C.              |
|------------|----------------------------|
| 8.00 A.M.  | = 23.5                     |
| 9.00 „     | = 24.0                     |
| 9.30 „     | = 24.5                     |
| 10.10 „    | = 25.75                    |
| 10.45 „    | = 26.00                    |
| 11.00 „    | = 25.75                    |
| 11.15 „    | = 25.9                     |
| 11.35 „    | = 26.0                     |
| 12.30 P.M. | = 26.0 (germination begun) |
| 12.45 „    | = 26.0                     |

But fell to 24.75 on opening to arrange.

I now found a rodlet 5 divisions long ( $= 5 \times 4.5 = 22.5 \mu$ , and began measuring.

Doubling periods :—

1. 22.5—45  $\mu$  = 12.45 P.M.—1.26 P.M. = 41 minutes at 25.0—26.0° C.  
 2. 45.0—90 „\* = 1.26 „ —2.10 „ = 44 „ 26.0—26.5° C.

This filament had another spore attached to it, and germinating there; possibly disturbances resulted, so I found another filament, and measured it as follows :—

Normal doubling period :—

36—72  $\mu$  = 1.10 P.M.—1.58 P.M. = 48 minutes at 25.8—26.4° C.

Or, calculated back :—

74.25—37  $\mu$  = 48 minutes at 25.8—26.4° C.

---

\* Calculated from curve.

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp.                |
|----------|---------|-----------|---------|---------|----------------------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . |
| 2.4 A.M. | 47.25   | —         | —       | —       | 26.5                 |
| 2.30 „   | 76.5    | 30        | 29.25   | 0.97    | 27.0                 |
| 2.45 „   | 90.0    | 15        | 13.5    | 0.9     | 27.0                 |
| 3.0 P.M. | 112.5   | 15        | 22.5    | 1.5     | 27.0                 |
| 3.20 „   | 150.75  | 20        | 38.25   | 1.9     | 27.1                 |
| 3.55 „   | 270.0   | 35        | 119.25  | 3.4     | 27.5                 |

Curve 112, p. 402.

Doubling periods:—

1. 47.25—94.5  $\mu$  = 2.4 P.M.—2.44 P.M. = 40 minutes at 26.5—27.0 $^{\circ}\text{C}$ .
2. 94.5—189.0 „ = 2.44 „ —3.32 „ = 47 „ 27.0—27.25 $^{\circ}\text{C}$ .

This was now too long to measure under this power, so I changed to the Zeiss C:—

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp.                |
|----------|---------|-----------|---------|---------|----------------------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . |
| 4.0 P.M. | 285.0   | —         | —       | —       | 27.1                 |
| 4.10 „   | 330.0   | 10        | 45.0    | 4.5     | 27.5                 |
| 4.20 „   | 382.5   | 10        | 52.5    | 5.2     | 27.5                 |

3. 189—378  $\mu$  = 3.32 P.M.—4.20 P.M. = 47 minutes at 27.1—27.5 $^{\circ}\text{C}$ .

Or, calculated back:—

- 382.5—191.25  $\mu$  = 48 minutes at 27.25—27.5 $^{\circ}\text{C}$ .
- 191.25—95.5 „ = 47 „ 27.00—27.25 $^{\circ}\text{C}$ .
- 95.5—47.75 „ = 42 „ 26.5—27.0 $^{\circ}\text{C}$ .

After 4.20, I let the temperature rise as follows:—

| Time.     | Temp.<br>$^{\circ}\text{C}$ . |
|-----------|-------------------------------|
| 4.30 P.M. | = 28.0                        |
| 4.45 „    | = 29.0                        |
| 8.0 „     | = 32.0                        |
| 8.25 „    | = 32.4                        |

And at 8.25 chose a segment to measure under the C (each division = 7.5  $\mu$ ) as follows:—

The doubling periods were as follows:—

1. 41.25—82.5  $\mu$  = 8.25 A.M.—9.20 A.M. = 55 minutes at 32.4—33.1—32.25 $^{\circ}\text{C}$ .

Or, taking 60  $\mu$  as the initial length:—

- 60—120  $\mu$  = 8.52 A.M.—10.10 A.M. = 78 minutes at 32.75—32.25—32.75 $^{\circ}\text{C}$ .

A somewhat remarkable fact was observed in these July 28 cultures. After standing all night at 32·75°, falling to 26° at 10 A.M. on the 29th July, both the cultures had passed over into spores.

That is to say, at temperatures between 23·5°, rapidly rising to 26°, then up to 33° C., slowly falling to 26° again, two cultures passed through their cycle from spore to spore in twenty-seven hours.

It would be interesting to investigate the properties of these spores thus rapidly formed at high temperatures, and compare them with others.

It is noteworthy that the culture of July 27, though treated exactly similarly from 8 A.M. 28th, to 10 A.M. 29th July, had as yet formed no spores. This latter had grown far more luxuriantly, however (at 27·5 on the 27th, see record), and served to show that the higher temperatures of the 28th culture, though driving the organism to more rapid spore formation, inhibit the growth, as the curve shows. It should also be noted that the spores formed were small, and, in many filaments, sparse, though otherwise normal to all appearance.

On July 29, spores, in broth + 1 per cent. gelatine were started at 10.15 A.M., the temperatures running as follows:—

| Time.        | Temp.<br>° C. |
|--------------|---------------|
| 10.15 A.M. = | 25·5          |
| 10.30 „ =    | 26·0          |
| 10.45 „ =    | 26·25         |
| 11.10 „ =    | 26·25         |
| 11.20 „ =    | 26·4          |
| 11.30 „ =    | 26·0          |
| 11.35 „ =    | 26·0          |
| 11.50 „ =    | 25·5          |
| 12.0 noon =  | 25·0          |
| 12.15 P.M. = | 24·8          |
| 12.25 „ =    | 25·25         |
| 12.40 „ =    | 25·6          |
| 1.0 „ =      | 25·8          |
| 1.5 „ =      | 26·0          |
| 2.25 „ =     | 26·0          |

I now sought for a filament, and was surprised to find what rapid progress had been made, for two were found of 67·5 and 70  $\mu$  respectively. The shorter was chosen for measurements.

Doubling period:—

1. 67·5—125  $\mu$  = 2.30 P.M.—3.7 P.M. = 37 minutes at 25·6—26·2° C
2. 125 —250 „ = 3.7 „ — 3.44 „ = 37 „ 26·2—25·75° C.

Doubling periods calculated back :—

276·75—138·5  $\mu$  = 3.50 P.M.—3.13 P.M. = 37 minutes at 26·25—25·75° C.  
 138·5 — 69·25 „ = 3.13 „ —2.40 „ = 33 „ 25·75—26·25° C.

Normal doubling period :—

110·25—221  $\mu$  = 3 P.M.—3.37 P.M. = 37 minutes at 26—26·25—26° C.

Evidently the temperatures were near the optimum.

On July 27 spores sown in broth + 1 per cent. gelatine were put in at 7.45, the temperatures running as follows :—

| Time.                 | Temp.<br>° C. |
|-----------------------|---------------|
| 7.45 A.M. =           | 19·5          |
| 8.0 „ =               | 20·25         |
| 9.0 „ =               | 23·1          |
| 9.45 „ =              | 24·5          |
| 10.10 „ =             | 24·8          |
| 10.30 „ =             | 25·4          |
| 11.7 „ =              | 26·0          |
| 11.40 „ =             | 26·75         |
| Fell to 26 on opening |               |
| 12.30 P.M. =          | 27·0          |
| 12.45 „ =             | 27·0          |
| 1.0 „ =               | 27·2          |
| 1.30 „ =              | 27·5          |
| 2.0 „ =               | 27·5          |

The measurements were now begun on a stout filament divided into two segments, which were both very straight, and formed an angle of about 150 with each other. It was easy to measure both segments, practically simultaneously, and so get not only the growth curve of each segment but also (as the sum of the two) that of the whole filament, as follows :—

#### *Segment 1.*

The doubling period here :—

78·75—157·5  $\mu$  in 39 minutes at 27·5—27·7° C.

Or, calculated back :—

234—127  $\mu$  = 2.25 P.M.—3.0 P.M. = 35 minutes at 27·6—27·75° C.

Normal doubling period :—

78·75—157·5  $\mu$  = 2.0 P.M.—2.39 P.M. = 35 minutes at 27·5—27·9° C.

Segment 2.

The doubling period here :—

72—144  $\mu$  in 38 minutes at 27·5—27·7° C.

Or, calculated back :—

225—112·5  $\mu$  = 2.24 P.M.—3.0 P.M. = 36 minutes at 27·6—27·8—27·75° C.

Normal doubling period :—

72—144  $\mu$  = 2.0 P.M.—2.38 P.M. = 38 minutes at 27·5—27·7° C.

Total.

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp. |
|----------|---------|-----------|---------|---------|-------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 2.0 P.M. | 150·75  | —         | —       | —       | 27·5  |
| 2.10 "   | 177·75  | 10        | 27      | 2·7     | 27·5  |
| 2.20 "   | 213·75  | 10        | 36      | 3·6     | 27·6  |
| 2.30 "   | 261·0   | 10        | 47·25   | 4·7     | 27·7  |
| 2.40 "   | 313·0   | 10        | 52·0    | 5·2     | 27·7  |
| 2.50 "   | 377·75  | 10        | 62·75   | 6·27    | 27·8  |
| 3.0 "    | 459·0   | 10        | 81·25   | 8·1     | 27·75 |

The following measurements were made on another filament in this culture :—

In this case the measurements were made with Zeiss C, each scale division = 7·8  $\mu$ , and I regard the numbers as not *quite* so accurate as preceding ones, partly owing to the amplification and partly to the extremely rapid growth.

The doubling period :—

140—298  $\mu$  in 35 minutes at 27—27·5° C.

Calculated back :—

343—172·5  $\mu$  = 40 minutes at 27·5—27·7° C.

This culture remained in the Sachs' box through all the next day (July 28) at 26—33°, and had not developed spores on the 29th at 10 A.M. It was returned to the box. On the 30th, spores were formed.

On August 3 spores sown in broth + 1 per cent. gelatine were put at 5.15 A.M. into the incubator at 23° C. to germinate. At 10.20 the Sachs' box was ready to receive them. Germination had been rapid and normal, and rods from 40 to 70  $\mu$  were present.

The culture was fixed at 10.25 at 24·5° C.

The temperatures during accommodation varied as follows :—

| Time.        | Temp.<br>° C. |
|--------------|---------------|
| 10.25 A.M. = | 24.5          |
| 10.27 „ =    | 26.0          |
| 10.28 „ =    | 26.5          |
| 10.29 „ =    | 26.8          |
| 10.30 „ =    | 27.0          |
| 10.36 „ =    | 27.4          |
| 10.45 „ =    | 27.4          |
| 10.55 „ =    | 28.3          |

At 11.2 the measurements began as follows :—

Normal doubling periods :—

1.  $60.75-121.5 \mu = 11.2$  A.M.—11.37 A.M. = 35 min. at  $28.0-28.5-28.3^{\circ}$  C.
2.  $121.5-243.0$  „ = 11.37 „ —12.15 P.M. = 38 „  $28.3-28.25-28.75^{\circ}$  C.

Or, calculated back :—

$252-126 \mu = 11.39$  A.M.—12.18 P.M. = 39 minutes at  $28.3-28.25-28.75^{\circ}$  C.  
 $126-63$  „ = 11.3 „ —11.39 A.M. = 36 „  $28.0-28.5-28.3^{\circ}$  C.

On August 2 a broth + 1 per cent. gelatine sowing of spores was put in at 8.30, the temperature as follows :—

| Time.               | Temp.<br>° C.             |
|---------------------|---------------------------|
| 8.30 A.M. =         | 21.3                      |
| 9.30 „ =            | 21.4                      |
| 10.0 „ =            | 21.3                      |
| 11.0 „ =            | 21.3                      |
| 11.15 „ =           | 21.5 (opened side window) |
| 12.0 noon =         | 21.25                     |
| 12.10 P.M. =        | 21.6 (opened window)      |
| 12.25 „ =           | 21.5                      |
| Swollen spore seen. |                           |
| 12.45 „ =           | 21.6                      |
| 1.0 „ =             | 21.6 Germ. = $3 \mu$      |
| 1.45 „ =            | 21.6 „ = $6 \mu$          |

The culture was now put on a slate slab at  $18^{\circ}$  C. until 4 P.M., when (the box temperature having been raised) it was put into culture at  $27.5^{\circ}$ , and raised to  $31.2^{\circ}$  by 4.40, and measurements made as follows :—

Doubling period :—

$135-270 \mu = 4.40$  P.M.—5.17 P.M. = 37 minutes at  $31.2-31.7^{\circ}$  C.

Or, reckoning back :—

$283-141.5 \mu = 4.42$  P.M.—5.20 P.M. = 38 minutes at  $31.25-31.75^{\circ}$  C.

Normal doubling period :—

135—270  $\mu$  = 4.40 P.M.—5.17 P.M. = 37 minutes at 31.2—31.7° C.

In resuming the foregoing series, we note that they fully bear out the previous conclusions, and are closely comparable with the broth-cultures.

Before proceeding to summarise further, it will be instructive to look at a series of experiments made *not with spores*, but with *segments of filaments* transferred bodily to the new media for measurement.

*D. Experiments with Growing Bacilli and Filaments in Broth + 1 per cent. Gelatine, and in Broth alone.*

On July 29 I tried the following experiment. Spores sown in broth + 1 per cent. gelatine in a tube were kept in incubator till 5 P.M. at 25° C. Then I fused the tube end, and thoroughly shook up the filaments to break them into segments. A drop was then used to infect a new tube, and from this a drop put into a cell and used to see if the growth measurements would correspond.

An excellent rod, 24.75  $\mu$  long, was easily found in the drop at 5.7 P.M. Giving this a little time to recover the shock of the violent shaking, and to accommodate itself to the temperature, food material, &c., I proceeded to measure it, noting that it began to grow at once, as follows :—

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp. |
|----------|---------|-----------|---------|---------|-------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 5.7 P.M. | 24.75   | —         | —       | —       | 25.25 |
| 5.15 "   | 25.87   | 8         | 1.12    | 0.1     | 25.7  |
| 5.20 "   | 26.9    | 5         | 1.12    | 0.2     | 25.9  |
| 5.25 "   | 28.0    | 5         | 1.12    | 0.2     | 26    |

A very slow, but distinctly perceptible growth.

The regular measurements were then begun at 5.30 P.M.

Normal doubling periods :—

1. 29.25—58.5  $\mu$  = 5.30 P.M.—6.15 P.M. = 45 minutes at 26—25.8—26° C.
2. 58.5—117 " = 6.15 " —6.55 " = 40 " 26.0—26.4° C.
3. 117—234 " = 6.55 " —7.34 " = 39 " 26.4—26° C.

It is clear that the rate of growth is the same, under the same conditions, as for the filaments normally germinated out from the spores, a point of some importance, as it may perhaps permit an extension of the method in several directions, besides a considerable saving of time.

It seems to me that a good deal of valuable information might be obtained by pursuing this method.

On July 30 I repeated this experiment with a rodlet from a *two days'* culture at 23—25°, shaken in its own liquor and then into fresh (broth + 1 per cent. gelatine in both cases). It was started at 3.25 at 24° C., and remained till 4 P.M. to accommodate itself to the new conditions, the temperature having risen to 24.6°.

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp. |
|----------|---------|-----------|---------|---------|-------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 4.8 P.M. | 25.5    | —         | —       | —       | 24.75 |
| 4.13 "   | "       | 5         | 0       | 0       | 24.5  |
| 4.21 "   | "       | 8         | 0       | 0       | 24.6  |
| 4.45 "   | "       | 24        | 0       | 0       | 24.4  |
| 4.54 "   | "       | 9         | 0       | 0       | 24.5  |
| 5.0 "    | "       | 6         | 0       | 0       | 25.0  |

I concluded that no growth occurred either (1) because it took a long time for bacilli beginning to pass over into spore formation to accommodate themselves; or (2) because the change of temperature was too sudden; or (3) because something was passed in of the nature of a poison. The shaken tube had remained some hours, and no bacilli in the drop were growing at 5 P.M.

On July 31 a broth + 1 per cent. gelatine tube, sown 8 A.M. on July 30, was shaken up after having been till 12.30 at 23—24° C.—i.e., 28½ hours—and a sowing made.

The temperature in Sachs' box was 20.5°, and rose as follows during accommodation period:—

| Time.      | Temp.<br>° C. |
|------------|---------------|
| 12.30 P.M. | = 20.5        |
| 12.40 "    | = 21.0        |
| 12.45 "    | = 21.75       |
| 12.50 "    | = 21.6        |
| 2.0 "      | = 21.0        |

I now satisfied myself the rodlets were growing, but they were floating about, and I could not fix one till much later.

| Time.     | Temp.<br>° C. |
|-----------|---------------|
| 2.25 P.M. | = 20.25       |
| 2.50 "    | = 21.0        |
| 3.30 "    | = 22.1        |
| 4.14 "    | = 23.25       |

One was fixed at 4.18 P.M., and measured as follows:—

Doubling periods (long beyond 1st normal):—

1. 54—108  $\mu$  = 4.18 P.M.—5.4 P.M. = 48 minutes at 23.5—23.75° C.
2. 108—216 „ = 5.4 „ —5.46 „ = 42 „ 23.75—24—23.75 C.

Or, calculating back:—

$$\begin{aligned} 279-130.5 \mu &= 5.20 \text{ P.M.}-6.0 \text{ P.M.} = 40 \text{ minutes at } 23.9-23.6^\circ \text{ C.} \\ 139.5-69.75 \mu &= 4.88 \text{ „}-5.20 \text{ „} = 42 \text{ „ } 23.5-24-23.9^\circ \text{ C.} \end{aligned}$$

Another rodlet was chosen at 6.30 P.M., as follows:—

The doubling period here:—

$$103.5-207 \mu = 6.33 \text{ P.M.}-7.20 \text{ P.M.} = 47 \text{ minutes at } 23.75-22.4-22.5^\circ \text{ C.}$$

Or, calculating back:—

$$230-115 \mu = 6.40 \text{ P.M.}-7.30 \text{ P.M.} = 50 \text{ minutes at } 22.65-22.4-22.6^\circ \text{ C.}$$

On August 1 a sowing was made of shaken rods from the tube (of rods) made on July 31, and which had stood 21½ hours at 18—20°.

The sowing was made at 9.30, and allowed till 2 P.M. to accommodate itself, at the following temperatures:—

| Time.      | Temp.<br>° C. |
|------------|---------------|
| 9.30 A.M.  | = 22.0        |
| 9.45 „     | = 22.0        |
| 10.25 „    | = 22.0        |
| 11.0 „     | = 22.0        |
| 11.30 „    | = 22.2        |
| 11.57 „    | = 22.0        |
| 12.10 P.M. | = 22.2        |
| 12.40 „    | = 21.7        |

At 2 P.M. a rod was selected and measured as follows:—

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp. |
|----------|---------|-----------|---------|---------|-------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 2.0 P.M. | 24.75   | —         | —       | —       | 20.5  |
| 2.10 „   | 27.0    | 10        | 2.25    | 0.22    | 20.3  |
| 2.20 „   | 29.25   | 10        | 2.25    | 0.22    | 20.4  |

but had to be abandoned. Another was selected at 5.30 as follows [for temperatures between, see spore culture, August 1] in two segments.

*Segment A.*

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp.         |
|-----------|---------|-----------|---------|---------|---------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 5.30 P.M. | 85.5    | —         | —       | —       | 22.5          |
| 5.40 "    | 91.75   | 10        | 6.25    | 6.2     | 22.25         |
| 5.50 "    | 103.0   | 10        | 11.25   | 1.12    | 22.3          |
| 6.0 "     | 112.0   | 10        | 9.0     | 0.9     | 22.4          |
| 6.10 "    | 130.0   | 10        | 18.0    | 1.8     | 22.5          |
| 6.20 "    | 148.0   | 10        | 18.0    | 1.8     | 22.5          |

Again I had to abandon the filament, owing to its awkward curvatures. Comparison with another filament enabled me to assure myself of the continued growth, however, at corresponding rates.

Doubling periods:—

85.5—171  $\mu$  = 5.30 P.M.—6.31 P.M. = 61 minutes at 22.5—22.25—22.5 $^{\circ}$  C.

*Segment B.*

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp.         |
|-----------|---------|-----------|---------|---------|---------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 5.30 P.M. | 81.0    | —         | —       | —       | 22.5          |
| 5.40 "    | 85.5    | 10        | 4.5     | 0.45    | 22.25         |
| 5.50 "    | 94.5    | 10        | 9.0     | 0.9     | 22.3          |
| 6.0 "     | 103.5   | 10        | 9.0     | 0.9     | 22.4          |
| 6.10 "    | 117.0   | 10        | 13.5    | 1.35    | 22.5          |
| 6.20 "    | 135.0   | 10        | 18.0    | 1.8     | 22.5          |

Doubling period:—

81—162  $\mu$  = 5.30 P.M.—6.32 P.M. = 62 minutes at 22.5—22.25—22.5 $^{\circ}$  C.

On August 2nd, rodlets, in broth for 9½ hours, at 23—24 $^{\circ}$ , were shaken and sown in broth, and put into dark Sachs' box at 5.30, rising from 27 $^{\circ}$  as follows—half an hour being given for accommodation.

| Time.     | Temp.<br>$^{\circ}$ C. |
|-----------|------------------------|
| 5.30 P.M. | = 27.0                 |
| 5.36 "    | = 29.5                 |
| 5.45 "    | = 30.3                 |
| 5.50 "    | = 30.7                 |

The rod for measuring was selected at 6 o'clock, the temperature falling a little during the search.

*Rodlet in Broth.*

Normal doubling periods:—

1.  $33.75-67.5 \mu = 6.5$  P.M.—6.37 P.M. = 32 minutes at  $29.6-29.5-30.5^{\circ}$  C.
2.  $67.5-135 \mu = 6.37$  „ —7.5 „ = 28 „  $30.5-30^{\circ}$  C.

Or, calculated back:—

$$153-76.5 \mu = 6.41 \text{ P.M.} - 7.10 \text{ P.M.} = 29 \text{ minutes at } 30.5-30^{\circ} \text{ C.}$$

On August 3, rodlets in broth—from spores sown at 5.15 A.M., at  $23^{\circ}$  C., and therefore  $7\frac{1}{2}$  hours growth—were shaken in broth and a culture put into Sachs' box at  $27-28^{\circ}$  C., at 12.45 P.M.

During accommodation, the temperatures varied as follows:—

| Time.      | Temp.<br>° C. |
|------------|---------------|
| 12.50 P.M. | = 26.75       |
| 12.52 „    | = 27.25       |
| 12.54 „    | = 27.5        |
| 12.59 „    | = 27.75       |
| 1.50 „     | = 27.5        |

At 1.54 P.M. the measurements began on a rod  $49.5 \mu$  long.

$$49.5-99 \mu = 1.54 \text{ P.M.} - 2.40 \text{ P.M.} = 46 \text{ minutes at } 27.2-28^{\circ} \text{ C.}$$

Or calculated back:—

$$141.75-71 \mu = 2.21 \text{ P.M.} - 3.0 \text{ P.M.} = 39 \text{ minutes at } 27.2-28.5^{\circ} \text{ C.}$$

Another rodlet, in two segments, was selected at 3 o'clock.

*Segment A.*

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp. |
|----------|---------|-----------|---------|---------|-------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 3.1 P.M. | 31.5    | —         | —       | —       | 28.5  |
| 3.10 „   | 37.25   | 9         | 6.75    | 0.75    | 29.0  |
| 3.23 „   | 49.5    | 13        | 12.25   | 0.9     | 29.0  |
| 3.30 „   | 54.0    | 7         | 4.5     | 0.6     | 29.0  |
| 3.40 „   | 63.0    | 10        | 9.0     | 0.9     | 28.8  |
| 3.50 „   | 67.5    | 10        | 4.5     | 0.45    | 28.6  |
| 4.33 „   | 67.5    | —         | —       | —       | 27.5  |

The growth had ceased entirely, and the filament was the same length as at last observation. Whether this was mere arrest, due to falling temperature (as is probable), or death, could not be determined.

Doubling period :—

$31.5-63 \mu = 3.1 \text{ P.M.}-3.40 \text{ P.M.} = 39 \text{ minutes at } 28.5-29-28.6^\circ \text{ C.}$

Or calculated back :—

$67.5-33.75 \mu = 3.5 \text{ P.M.}-3.50 \text{ P.M.} = 45 \text{ minutes at } 28.75-29-28.6^\circ \text{ C.}$

*Segment B.*

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp.              |
|----------|---------|-----------|---------|---------|--------------------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^\circ \text{C.}$ |
| 3.1 P.M. | 90.0    | —         | —       | —       | 28.5               |
| 3.10 "   | 108.0   | 9         | 18.0    | 2.0     | 29.0               |
| 3.23 "   | 135.0   | 12        | 27.0    | 2.0     | 29.0               |
| 3.30 "   | 160.75  | 7         | 15.75   | 2.2     | 29.0               |
| 3.40 "   | 171.0   | 10        | 11.25   | 1.1     | 28.8               |
| 3.50 "   | 189.0   | 10        | 18.0    | 1.8     | 28.6               |
| 4.33 "   | 189.0   | —         | —       | —       | 27.5               |

The growth had ceased (as said under segment A).

Doubling period :—

$90-180 \mu = 3.1 \text{ P.M.}-3.45 \text{ P.M.} = 44 \text{ minutes at } 28.5-29-28.7^\circ \text{ C.}$

Or calculated back :—

$189-94.5 \mu = 3.3 \text{ P.M.}-3.50 \text{ P.M.} = 47 \text{ minutes at } 28.5-29-28.7^\circ \text{ C.}$

If we now summarise all these results, it is seen that there are three points to be considered as regards the growth and behaviour of the organism, (1) The rate of germination: (2) the rapidity of post-germinal growth,—or, what is here the same thing, doubling period, and (3) the time occupied in completing the life-cycle from spore to spore. The labour involved in making out all these points accurately would be enormous, and I have only been able to concentrate my attention on the growth curves, and to go accurately into the doubling periods; though, as we see, some interesting data are given concerning the other phenomena, wherever they could be noted.

Secondly, it will be seen that while paying especial attention to the one point—the influence of temperature on the doubling period—I had occasion to test to some extent the effects of altering the medium in which the growth occurs, and the results are sufficient to show that here, again, there is a large and interesting field for enquiry.

Thirdly, the question arose as to whether any difference is observable between the behaviour of filaments, grown *direct from the spore*, in a given medium at a given temperature, and those grown *from a segment of greater age*; and although here again the enormous

labour involved would be too great to charge myself with at present, sufficient facts came to hand to show that this branch of the enquiry also ought to be taken up and pursued.

Fourthly, the point arose by the way how far the doubling period at a given temperature, and in a given medium, is affected by whether the filament measured has developed all along,—i.e., had germinated—at the temperature of the growth measurements, or whether germination was conducted at some other temperature, and the growing filament then brought into the new temperature.

A fifth point arises by the way also. How far is the matter affected by the age of the food-medium?

#### *Curves of the Doubling Periods.*

If we plot out the results reached so far, some interesting curves can be obtained which help us materially.

Taking any series—say the cultures in weak gelatine—the *averages* of all the doubling periods obtained for the several temperatures may be plotted on ordinates erected on a base line divided into degrees centigrade, and on joining the points thus plotted we get a curve convex to the base line. As a matter of fact, however, the curve so obtained from averages of *all* doubling periods is very irregular, and all that one can say is that, (1) generally speaking, the curve descends from somewhat high numbers below 18° C., to the lowest numbers between 25—30°, and then ascends again.

2. The various jerks on this curve are unequal, and without any evident order, and the re-ascent of the curve beyond 30° C. is not always obtained.

3. On the whole the curve descends most for broth, and least for weak gelatine.

It seems hardly worth while to go more into details, since it was so evident that something had to be explained here, before further steps could be taken, that I made a careful analysis of all the conditions, and found that the irregularities on these curves of all averages were due to a number of more or less controllable causes.

In the first place, it turns out that it was a serious error on my part to start the germination of the spores in the Sachs' box at various convenient temperatures with reference to the temperatures of observation, and many of the irregularities in question are due to the consequent (1) differences in initial vigour of the plantlets started at different temperatures, (2) differences in *phase* of growth of such rodlets, and (3) differences in degree of exhaustion of the food-material in the drops, which had thus been exposed for different lengths of time to different initial temperatures.

It is hardly necessary to go into the details of this very complex

matter, which I had to test in various ways before deciding on a satisfactory plan—for it must be borne in mind that one has to so arrange matters that the necessary phase of growth shall occur at a time convenient for observation and measurement, otherwise it would be imperative on one to be ready to take the measurable phase at any hour of the day or night—it may therefore suffice to say that further reflection on all the circumstances suggests that the following conditions would have to be fulfilled before satisfactory results could be obtained. It will be obvious that some of these conditions can *not* be completely satisfied, so that observers will have to be content to approach them as nearly as possible. I give the ideas as they arise, but perhaps other investigators can suggest improvements.

1. The temperatures employed should be constant, and this condition can be so nearly approached that we may regard practice as according with theory. The difficulties have been sufficiently discussed as they arose.

2. The food-material and medium employed should be constant. This condition can also be approximately carried out, because although (1) the water-contents of the hanging-drop vary at different temperatures, and (2) no food-medium remains exactly the same from day to day, however carefully prepared and kept, considerable accuracy can be attained. This difficulty cannot be got over by using a fresh brew each time, because no two brews can be *exactly* alike. With great care, the nearest approximation to uniformity must be aimed at.

3. The spores used must be from the same culture. This can be realised, and, apart from theoretical differences as the age of spores increases, we may regard this condition as satisfied.

4. The culture-cells used must be prepared in the same way, and be loaded with the same water supply, and have the same air-contents. Practically this condition is also met.

5. The culture-drops should always be of the same size, and supply equal amounts of food-materials. Here we cannot do more than approximate as closely as possible to the requirements, though much can be done by practice, using the same loop, and so on.

6. Strictly speaking, the drop ought always to contain the same number of spores—preferably, no doubt, *one* spore—but it would be impossible to meet this condition exactly, in working with large numbers of cultures, as I have to do, and so approximation only can be hoped for. I regard the matter of importance, however, for it is impossible to overlook the probability—amounting to all but certainty—that each filament is to some extent an antagonist to all its neighbours: competing with them for oxygen, water, and food-materials, and, no doubt, affecting the medium injuriously as its metabolism proceeds. With great care and long practice it is

possible to make the sowings approximately uniform—say 12–25 spores in each drop. More can hardly be hoped for.

7. The spores ought always to be germinated out at the same *constant* temperature, though beyond the confirmation of the fact that some temperatures favour germination better than others, my experiments throw little light or further details on this matter. I select 22° C. as a useful temperature.

8. It seems probable from the foregoing that the germinated culture ought always to be transferred to the temperature, &c., at which the growth is to be examined *at the same age*, so that the filaments to be compared, as to their growth-curves under the new conditions, shall have been exposed to uniform conditions for the same period of time previous to the study of the new reactions, and so that we may always select filaments of *equal length and age* from the spore, for comparison. This is undoubtedly a difficult condition to fulfil, and I have by no means always succeeded in fulfilling it, because it requires that the investigator shall be able to begin his work at any stated times.

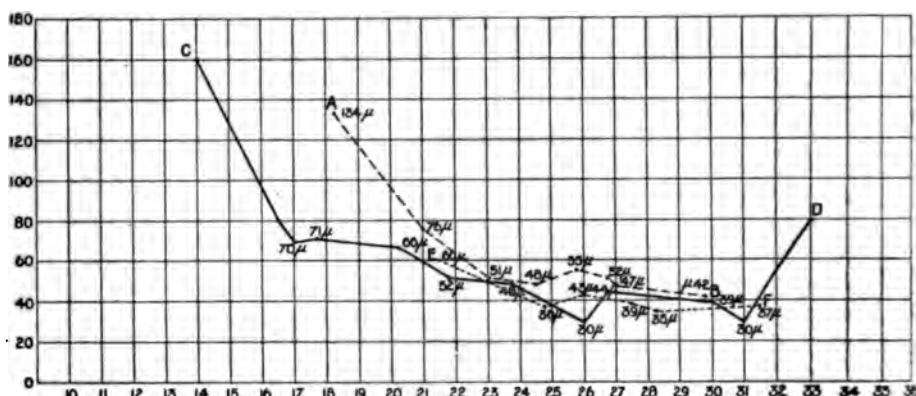
9. Lastly comes the question of what we shall compare on the growth-curves. The outcome of the experiments given suggests that it may not be sufficient to compare *any two* doubling periods, as has been done. Probably it would be better to decide to compare either (1) the doubling of equal lengths—*e.g.*, the elongation from 100 to 200  $\mu$ —at the given temperature, &c., or (2) the doubling period starting always at the same age—*e.g.*, whatever the length of the filament after one hour's sojourn at the given temperature, see how long it takes to double that length. Clearly we cannot do *both* with the same series of cultures, though perhaps the ideal to be aimed at is that both be done eventually. Looking at the importance of the subject, it seems worth while to throw out the suggestion at any rate.

The following set of cultures in normal gelatine show very clearly that excellent and clear results are to be obtained by observing the above precautions as closely as possible, and adopting the method of recording now to be described.

1. The culture, when put into the Sachs' box, is allowed half-an-hour for the organism to accommodate itself to the new temperature.

2. The first doubling period selected is that starting from the half-hour accommodation just mentioned, and all previous growth (*i.e.*, growth during the accommodation period) is neglected. I term these doubling periods "normal doubling periods," in contradistinction to the foregoing doubling periods selected anywhere on the curves.

3. If we plot out these *normal* doubling periods, as already described (Curves AB, CD, and EF), we obtain a much more regular curve, which, as will be seen, comes evidently into the category of respiration curves.



On examining these three curves, AB, CD, and EF,\* we see that—

1. The general tendency of the weak gelatine—i.e., poor in food materials but stiff in consistency (Curve AB)—is to lengthen the doubling period.

2. That considerable irregularities occur at all the temperatures higher than about 24° C. or 25° C. These are no doubt due to the inequalities in the *phase of growth*, different ages of the culture, and so on, already discussed.

3. It looks as if the curve suddenly shot up at temperatures beyond 31—32° C., as shown by the curve for broth (CD).

4. It might, perhaps, be suggested that although the addition of gelatine delays the doubling period, it also delays the attainment of the maximum temperature, i.e., it protects the food-drop against exhaustion, until a higher temperature than that at which exhaustion sets in when broth alone is used. This may be the case, but I do not regard it as proved, and there are many difficulties with broth-cultures at higher temperatures, as already said.

But (5) it is clear that these three curves, unsatisfactory though they are as yet in detail, show striking general resemblances to a curve of respiratory activity—the steady fall in the doubling period as we pass from lower temperatures, and the sudden rise again beyond 31° C. (seen in the broth curve) being very suggestive—and I believe that is what they really are, for reasons already discussed.

Having so far cleared the way for the understanding of these curves, we shall see that when attention is carefully given to all the points discussed, a much more regular curve is obtained, as the following series show.

\* Curve AB summarises the averages for weak gelatine; CD, those for broth + 1 per cent. gelatine; EF, those for broth alone. The ordinates are minutes and the abscissae temperatures.

*Cultures in Normal 10 per cent. Gelatine.**Experiments on the effects of Temperature in the Dark, with constant Food-material and conditions of Germination.*

In the following series I used throughout the same normal gelatine—10 per cent. gelatine, with broth and peptone—and always incubated the spores at 22° C. until germination was completed and the rodlets in full growth. As far as possible, moreover, I started the growth-measurements on filaments in exactly the same phase of growth, and used drops of as nearly as possible the same size, and containing the same number of spores, though it was found to be quite impracticable to fulfil all these conditions absolutely.

The arrangements—Sachs' box, cells, temperature regulations, and conditions of measurement—were as before.

I select a number of typical curves from a much larger number, and need only say that they are quite representative and fair examples. The labour of recording the growths, working out the tables, and drawing the curves has been very great, as will readily be understood; but it seems worth while to have done it to get the insights here obtained into the marvellous regularity of growth that goes on when the conditions are under control, quite apart from the other information conveyed by the results.

Still, it will be obvious that it has been impossible to carry out more than one series so thoroughly in the time, and it must remain for others to investigate the behaviour of the organism in other media or under other conditions similarly controlled.

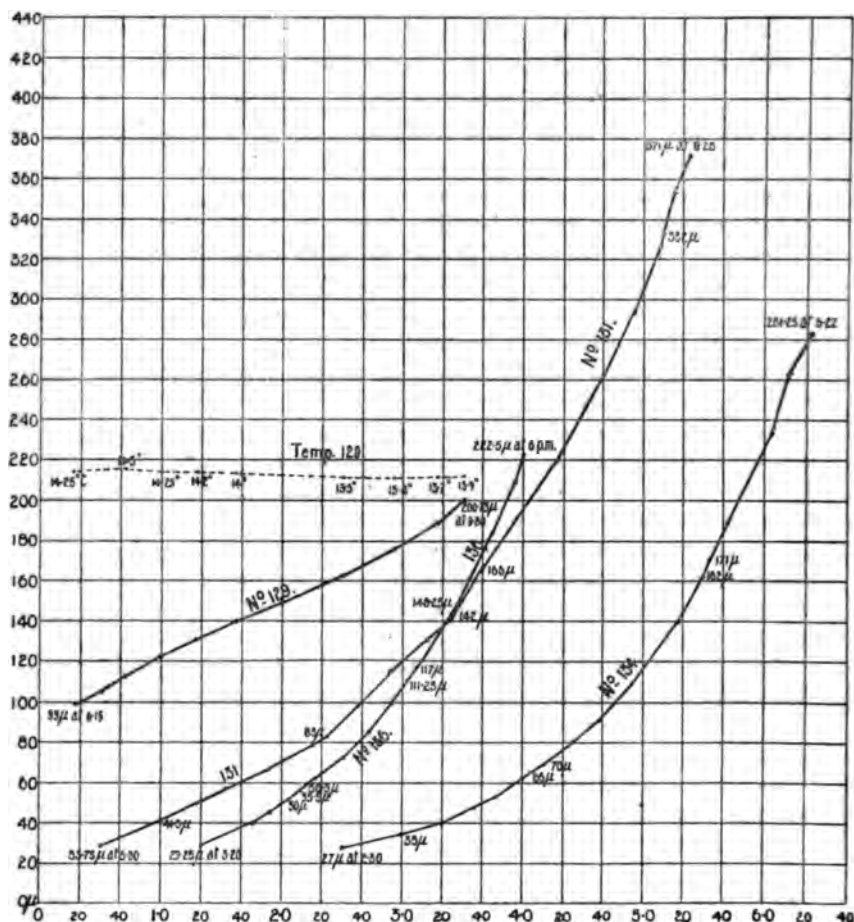
On November 16 spores were sown in normal gelatine at 10 A.M., and put at 22° C. till 6 P.M., and then put into the Sachs' box at 14° C., and measurements made as follows:—

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 6.15 P.M. | 99.0    | —         | —       | —       | 14.25 |
| 6.42 "    | 112.5   | 27        | 13.5    | 0.50    | 14.5  |
| 7.0 "     | 122.5   | 18        | 10.0    | 0.55    | 14.25 |
| 7.17 "    | 131.0   | 17        | 8.5     | 0.50    | 14.2  |
| 7.35 "    | 140.0   | 17        | 9.0     | 0.53    | 14.0  |
| 8.29 "    | 162.5   | 54        | 22.5    | 0.41    | 13.5  |
| 8.55 "    | 176.0   | 26        | 18.5    | 0.52    | 13.4  |
| 9.18 "    | 191.75  | 23        | 15.75   | 0.68    | 13.7  |
| 9.30 "    | 200.75  | 12        | 9.0     | 0.75    | 13.9  |

Curve 129, p. 434.

Doubling period:—

99—198  $\mu$  = 6.15 P.M.—9.27 P.M. = 192 minutes at 14.25—14.5—13.4—13.9° C.



On December 11 spores in normal gelatine were put in at 22° at 1.45, and into measurement at 6.15 P.M.

The normal doubling period :—

$$20.25-41 \mu = 6.45 \text{ P.M.} - 9.2 \text{ P.M.} = 137 \text{ minutes at } 14.5-15.1^{\circ} \text{ C.}$$

It is a very long and tedious business to work out these slow growths at low temperatures, but several other curves show that about 200 minutes represents the doubling period for 14° C.; the time rises very quickly as the temperature falls, however, and I find it is nearly 400 minutes for 10—12° C., and probably nearly 800 to 1000 minutes for 6—8°. This is, no doubt, partly due to the stiffness of the gelatine.

On December 10 spores were sown in normal gelatine at 22° C. at 10.30, and put into measurement at 3.30.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 3.30 P.M. | 33·75   | —         | —       | —       | 16·8  |
| 3.45 "    | —       | —         | —       | —       | 17·0  |
| 4·0 "     | 41·5    | 30        | 7·75    | 0·26    | 17·0  |
| 4.18 "    | 49·5    | 18        | 8·0     | 0·44    | 17·0  |
| 4.32 "    | 56·25   | 14        | 6·75    | 0·48    | 17·0  |
| 4.54 "    | 67·5    | 22        | 11·25   | 0·51    | 17·0  |
| 5.16 "    | 78·75   | 20        | 11·25   | 0·56    | 17·0  |
| 5.39 "    | 99·0    | 23        | 20·25   | 0·9     | 17·0  |
| 5.54 "    | 114·75  | 15        | 15·75   | 1·0     | 17·0  |
| 6.13 "    | 130·75  | 19        | 16·0    | 0·8     | 17·0  |
| 6.24 "    | 142·0   | 11        | 11·25   | 1·0     | 17·0  |
| 7.16 "    | 218·5   | 52        | 76·5    | 1·5     | 16·5  |
| 7.40 "    | 261·25  | 24        | 42·75   | 1·78    | 16·25 |
| 7.56 "    | 295·0   | 16        | 33·75   | 2·1     | 16·7  |
| 8.8 "     | 326·5   | 12        | 31·5    | 2·6     | 17·5  |
| 8.15 "    | 353·5   | 7         | 27·0    | 3·8     | 18·0  |
| 8.23 "    | 371·0   | 8         | 18·0    | 2·2     | 18·25 |

Curve 131, p. 434.

The doubling periods are (normal) as follows:—

41·5—83  $\mu$  = 4·0 P.M.—5.21 P.M. = 81 minutes at 17° C.

83·0—166 " = 5.21 " —6.40 " = 79 " 17° C.

166·0—332 " = 6.40 " —8.10 " = 90 " 17—16·25—17·5° C.

This shows very clearly the steady growth and equal doubling periods at constant temperature, and, at the same time, how slight an alteration in the temperature (causing variation either way) suffices to slow the growth.

Another segment in the same drop was now selected, and measured as follows:—

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 8.33 P.M. | 56·0    | —         | —       | —       | 18·0  |
| 8.40 "    | 59·5    | 7         | 3·5     | 0·5     | 18·0  |
| 8.51 "    | 67·0    | 11        | 7·5     | 0·7     | 18·1  |
| 9.0 "     | 72·0    | 9         | 5·0     | 0·55    | 18·0  |
| 9.10 "    | 78·75   | 10        | 6·75    | 0·67    | 18·0  |
| 9.20 "    | 87·75   | 10        | 9·0     | 0·9     | 17·8  |
| 9.30 "    | 95·5    | 10        | 7·75    | 0·77    | 17·6  |
| 9.41 "    | 103·5   | 10        | 8·0     | 0·8     | 17·5  |
| 9.50 "    | 112·5   | 9         | 9·0     | 1·0     | 17·5  |
| 10.0 "    | 122·5   | 10        | 10·0    | 1·0     | 17·5  |
| 10.10 "   | 132·5   | 10        | 10·0    | 1·0     | 17·5  |
| 10.21 "   | 144·0   | 11        | 11·5    | 1·1     | 17·6  |
| 10.30 "   | 153·0   | 9         | 9·0     | 1·0     | 17·6  |

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Here the doubling period was :—

56—112  $\mu$  = 8.33 P.M.—9.49 P.M. = 76 minutes at 18—18.1—17.5° C.

On November 17 spores sown as before, remained at 22° C., from 10.30 A.M. till 3.45, and were then put in at 19.5° C.

Doubling period :—

81.0—162  $\mu$  = 4.5 P.M.—5.16 P.M. = 71 minutes at 19.8—19.6—19.7—19.3° C.  
162.0—324 „ = 5.16 „ —6.26 „ = 70 „ 19.3—18.75—19.5° C.

Calculated back :—

459.0—229.5  $\mu$  = 5.49 P.M.—7.11 P.M. = 82 minutes at 19—18.75—19.5° C.  
229.5—114.5 „ = 4.37 „ —5.49 „ = 72 „ 19.7—19° C.

Normal doubling period :—

91.0—182.0  $\mu$  = 4.15 P.M.—5.26 P.M. = 71 m. at 19.7—19.6—19.7—19.6—19.3° C.  
182.0—364.0 „ = 5.26 „ —6.41 „ = 75 „ 19.8—18.75—19.5° C.

On December 8 spores were sown in normal gelatine at 11 A.M., and put into the incubator at 22° C. till 2.30 P.M.

| Time.     | Length.  | Interval. | Growth. | Rate.   | Temp. |
|-----------|----------|-----------|---------|---------|-------|
|           | $\mu$ .  | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 2.30 P.M. | 27 $\mu$ | —         | —       | —       | 19.5  |
| 2.35 „    | —        | —         | —       | —       | 19.0  |
| 3.20 „    | 40.5     | 50        | 13.5    | 0.27    | 18.5  |
| 3.30 „    | —        | —         | —       | —       | 19.0  |
| 3.36 „    | 40.5     | 16        | 9.0     | 0.56    | 19.0  |
| 3.47 „    | 54.0     | 11        | 4.5     | 0.4     | 19.0  |
| 4.0 „     | 63.0     | 13        | 9.0     | 0.7     | 19.0  |
| 4.10 „    | 69.75    | 10        | 6.75    | 0.67    | 19.0  |
| 4.25 „    | 81.0     | 15        | 11.25   | 0.75    | 19.0  |
| 4.38 „    | 92.25    | 13        | 11.25   | 0.86    | 18.9  |
| 4.55 „    | 110.25   | 17        | 18.0    | 1.0     | 19.0  |
| 5.17 „    | 139.5    | 22        | 29.25   | 1.33    | 19.4  |
| 5.33 „    | 171.0    | 16        | 31.5    | 2.0     | 19.6  |
| 5.42 „    | 189.0    | 9         | 18.0    | 2.0     | 19.5  |
| 6.6 „     | 236.25   | 24        | 47.25   | 2.0     | 19.25 |
| 6.15 „    | 263.25   | 11        | 27.0    | 2.4     | 19.25 |
| 6.22 „    | 281.25   | 7         | 18.0    | 2.6     | 19.3  |

Curve 134, p. 434.

The normal doubling periods :—

35—70  $\mu$  = 3.0 P.M.—4.10 P.M. = 70 minutes at 19.0—18.5—19.0° C.  
70—140 „ = 4.10 „ —5.18 „ = 68 „ 19.0—18.9—19.4° C.  
140—280 „ = 5.18 „ —6.21 „ = 63 „ 19.4—19.6—19.25—19.3° C.

On comparing these periods with those of November 17, it will be seen how admirably the agreement comes out, and further shows that

the food-material has not degenerated in quality during the interval of nearly a month, for it had remained in the flask for that interval.

From 6.15 onwards the above filament was segmented into a short and a long segment; in the following table I resume the further growth of the shorter one.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp.         |
|-----------|---------|-----------|---------|---------|---------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 6.15 P.M. | 65.0    | —         | —       | —       | 19.25         |
| 6.22 "    | 70.0    | 7         | 5.0     | 0.7     | 19.25         |
| 6.35 "    | 81.0    | 13        | 11.0    | 0.8     | 19.5          |
| 6.45 "    | 90.0    | 10        | 9.0     | 0.9     | 19.5          |
| 6.56 "    | 100.0   | 11        | 10.0    | 0.9     | 19.25         |
| 7.6 "     | 109.0   | 10        | 9.0     | 0.9     | 19.0          |
| 7.17 "    | 121.5   | 11        | 12.5    | 1.1     | 19.0          |
| 7.25 "    | 132.75  | 8         | 11.25   | 1.4     | 19.0          |

Here the doubling :—

65—130  $\mu$  = 6.15 P.M.—7.23 P.M. = 68 min. at 19.25—19.5—19.0 $^{\circ}$  C.

At 8.15 another segment in the same drop was chosen, and measured as follows :—

Some interesting comparisons are possible here. For instance, the original filament was 65  $\mu$  long at 4.3 P.M., and it was 109  $\mu$  long at 4.54 P.M.; the present segment was 65  $\mu$  long at 6.15, and was 109  $\mu$  long at 7.6. In both cases, therefore, exactly the same time, i.e., fifty-one minutes, was occupied in growing the 44  $\mu$  referred to, in the first case the temperature being 19 $^{\circ}$  to 18.9 $^{\circ}$  C., and in the second 19.25—19.5—19 $^{\circ}$  C.

Then, again, the doubling period is the same as the second one of the original filament, viz., sixty-eight minutes, at nearly the same temperature.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp.         |
|-----------|---------|-----------|---------|---------|---------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 8.17 A.M. | 114.75  | —         | —       | —       | 19.0          |
| 8.35 "    | 123.75  | 17        | 9.0     | 0.5     | 19.2          |
| 8.57 "    | 135.0   | 22        | 11.25   | 0.5     | 19.4          |
| 9.10 "    | 144.0   | 13        | 9.0     | 0.7     | 19.25         |
| 9.27 "    | 157.5   | 17        | 13.5    | 0.8     | 19.25         |
| 9.57 "    | 180.0   | 30        | 22.5    | 0.75    | 19.75         |
| 10.10 "   | 198.0   | 13        | 18.0    | 1.4     | 19.3          |

Here the doubling period is becoming extended to more than double the previous ones, indicating the spoiling of the drop either

by exhaustion or by excreta; the latter is more probable, because much growth went on during the next 24 hours, as examination next day showed.

On November 24 spores were sown in normal gelatine at 10 A.M. and kept at 22° C. till 3.15.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 3.20 P.M. | 29.25   | —         | —       | —       | 21.6  |
| 3.30 "    | —       | —         | —       | —       | 21.75 |
| 3.45 "    | 40.5    | 25        | 11.25   | 0.45    | 21.6  |
| 3.53 "    | 45.0    | 8         | 4.5     | 0.5     | 21.6  |
| 4.2 "     | 51.75   | 9         | 6.75    | 0.75    | 21.6  |
| 4.17 "    | 63.0    | 15        | 11.25   | 0.75    | 21.6  |
| 4.30 "    | 74.25   | 13        | 11.25   | 0.86    | 21.5  |
| 4.42 "    | 85.5    | 12        | 11.25   | 0.95    | 21.4  |
| 4.55 "    | 100.0   | 13        | 14.5    | 1.1     | 21.5  |
| 5.10 "    | 121.25  | 15        | 21.25   | 1.4     | 21.6  |
| 5.27 "    | 148.25  | 17        | 27.0    | 1.6     | 21.7  |
| 5.45 "    | 184.25  | 18        | 36.0    | 2.0     | 21.5  |
| 5.56 "    | 211.25  | 11        | 27.0    | 2.4     | 21.5  |
| 6.0 "     | 222.5   | 4         | 11.25   | 2.8     | 21.5  |

Curve 136, p. 434.

Doubling periods :—

29.25—58.5  $\mu$  = 3.20 P.M.—4.12 P.M. = 52 minutes at 21.6—21.75—24.6° C.  
 58.5—117.0 „ = 4.12 „ —5.7 „ = 55 „ 21.6—21.4—21.6° C.

Calculated back :—

222.5—111.25  $\mu$  = 5.2 P.M.—6.0 P.M. = 58 minutes at 21.6—21.7—21.5° C.  
 111.25—55.5 „ = 4.8 „ —5.2 „ = 54 „ 21.6—21.4—21.6° C.

Normal doubling periods :—

40.5—90  $\mu$  = 3.45 P.M.—4.46 P.M. = 61 minutes at 21.6—21.4° C.—  
 90.0—180 „ = 4.46 „ —5.43 „ = 57 „ 21.4—21.7—21.5° C.

Another culture, November 24, was from spores at 22° C. from 1 to 6 P.M.

Doubling periods :—

18—36  $\mu$  = 6.0 P.M.—7.0 P.M. = 60 minutes at 21.9—21.75° C.  
 36—72 „ = 7.0 „ —8.11 „ = 71 „ 21.75—21.9° C.  
 72—144 „ = 8.11 „ —9.41 „ = 90 „ 21.90° C.

Normal doubling period :—

27—54  $\mu$  = 6.30 P.M.—7.42 P.M. = 82 minutes at 21.75—21.9° C.

This filament was then left until 8.45, and the following further measurements taken :—

Doubling (second normal doubling period) :—

54—108  $\mu$  = 7.42 P.M.— 9.4 P.M. = 82 minutes at 21.9° C.  
108—216 „ = 9.4 „ —10.31 „ = 87 „ 21.9—21.8—21.9° C.

The foregoing cultures and their growth-curves are extremely interesting and instructive, especially because the temperatures were fortunately so constant.

As we see, the normal doubling period at 21.6°—21.4° was sixty-one minutes in the first case, where the germination period occupied five and a quarter hours, but it was seventy-two minutes (at 21.75—21.9°) in the second case, with a germination period of five hours and a shorter filament. The question arises, Why did the second culture occupy eleven minutes longer in doubling its shorter filament, although it had been exposed a quarter of an hour less time to the 22° C. of germination, and although the temperature was nearer the optimum by 0.3° to 0.25° C.?

I think the explanation is that the temperatures further from the optimum were brought to bear at an earlier stage on a relatively weaker plant, and the latter therefore coped less ably with the more unfavourable circumstances.

Turning again to the second culture, it is worth noting that its second normal doubling period, *at perfectly constant temperature*, be it noted, is distinctly longer than its own first one—eighty-two minutes against seventy-two—bearing out my previous remarks as to the gradual slowing of growth as the culture ages, and, no doubt, due to the slow exhaustion of the food-materials and fouling of the medium by products of metabolism.

On November 27 spores in normal gelatine were sown at 2.15 and put at 22° C. till 8.5 P.M.

Doubling periods :—

114.5—229.0  $\mu$  = 8.5 P.M.—8.55 P.M. = 50 minutes at 22.75—22.25° C.  
229.0—458.0 „ = 8.55 „ —9.48 „ = 53 „ 22.25—23.0° C.

Calculated back :—

454.5 —227.25  $\mu$  = 8.55 P.M.—9.48 P.M. = 53 minutes at 22.25—23.0° C.  
227.25—113.75 „ = 8.4 „ —8.55 „ = 51 „ 22.75—22.25° C.

Normal doubling period :—

176.0—342.0  $\mu$  = 8.35 P.M.—9.27 P.M. = 52 minutes at 22.5—23.0° C.

Spores sown November 26 at 12.45 in normal gelatine at 22° C., remained till 7.45 P.M. and were then measured at 23°.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temperature.         |
|-----------|---------|-----------|---------|---------|----------------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}\text{C}$ . |
| 7.40 P.M. | 218.0   | —         | —       | —       | 23.0                 |
| 8.3       | 274.5   | 14        | 56.5    | 4.0     | 23.0                 |
| 8.20 "    | 333.0   | 17        | 58.5    | 3.4     | 23.5                 |
| 8.34 "    | 396.0   | 14        | 63.0    | 4.5     | 23.5                 |
| 8.54 "    | 517.5   | 20        | 121.5   | 6.0     | 23.5                 |

The rate of growth was now so rapid I could no longer measure.  
Doublings:—

218.0—496.0  $\mu$  = 7.40 P.M.—8.41 P.M. = 52 minutes at 23.0—23.5 $^{\circ}$  C.

And back:—

517.5—258.75  $\mu$  = 7.59 P.M.—8.54 P.M. = 55 minutes at 23.0—23.5 $^{\circ}$  C.

On November 23 spores in normal gelatine were put in at 22 $^{\circ}$  C. at 2 P.M., and observations at 25 $^{\circ}$  started at 6 P.M.

Doubling:—

18.0—36.0  $\mu$  = 6.5 P.M.—6.39 P.M. = 34 minutes at 23.5—25.0—24.75 $^{\circ}$  C.

36.0—72.0 „ = 6.39 „ —7.30 „ = 51 „ 24.75—25.0 $^{\circ}$  C.

Calculated back:—

87.5—43.75  $\mu$  = 6.56 P.M.—7.40 P.M. = 44 minutes at 24.75—25.0 $^{\circ}$  C.

43.75—21.75 „ = 6.16 „ —6.56 „ = 40 „ 24.75—25.0—24.75 $^{\circ}$  C.

Normal doubling period:—

28.0—56.0  $\mu$  = 6.30 P.M.—7.16 P.M. = 46 minutes at 25.0—24.75—24.9 $^{\circ}$  C.

Another culture, made at 2 P.M. and hitherto at 22 $^{\circ}$  C., was put in at 8.40 P.M.

Doubling:—

195.75—391.5  $\mu$  = 8.45 P.M.—9.31 P.M. = 46 minutes at 23—25 $^{\circ}$  C.

Back:—

490.5—245.25  $\mu$  = 9.2 P.M.—9.45 P.M. = 43 minutes at 24.6—25.25—25.1 $^{\circ}$  C.

Normal doubling period:—

245.5—490.5  $\mu$  = 9.2 P.M.—9.45 P.M. = 43 minutes at 24.6—25.25—25.1 $^{\circ}$  C.

A curious phenomenon is often observed in these stiff gelatine cultures at temperatures near 23—24 $^{\circ}$  C. This is the neighbourhood of the melting point, and it sometimes happens that *as the drop melts the rate of growth rises quite sharply*; I explain this as due to the sudden access of more oxygen and available food-materials to the rodlets.

On November 12 spores were sown as before at 10.30, kept at 22° C. till 3.30, when into Sachs' box at 24·5° C.

Doubling :—

20·25—40·5  $\mu$  = 3.30 P.M.—4.10 P.M. = 40 minutes at 23·0—25·0° C.

Back :—

47·25—23·5  $\mu$  = 3.37 P.M.—4.22 P.M. = 45 minutes at 24·75—25·0° C.

Another culture, exactly as before at 22° since 10.30, was put in at 4.30.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 4.39 P.M. | 78·75   | —         | —       | —       | 24·25 |
| 4.48 "    | 90·0    | 9         | 11·25   | 1·2     | 24·6  |
| 5.8 "     | 135·0   | 20        | 45·0    | 2·2     | 24·6  |
| 5.15 "    | 155·25  | 7         | 20·25   | 2·9     | 24·75 |
| 5.25 "    | 177·75  | 10        | 22·5    | 2·2     | 25·0  |
| 5.31 "    | 202·5   | 6         | 24·75   | 4·1     | 25·0  |
| 5.37 "    | 220·5   | 6         | 18·0    | 3·0     | 25·0  |
| 5.44 "    | 245·25  | 7         | 24·75   | 3·5     | 25·0  |
| 5.54 "    | 292·5   | 10        | 47·25   | 4·7     | 25·1  |
| 6.1 "     | 328·5   | 7         | 36·0    | 5·0     | 25·2  |
| 6.6 "     | 360·0   | 5         | 31·5    | 6·3     | 25·4  |
| 6.12 "    | 396·0   | 6         | 36·0    | 6·0     | 25·5  |
| 6.17 "    | 423·0   | 5         | 27·0    | 5·4     | 25·5  |

Curve 142, p. 442.

I had now to stop, the filament was too long to measure further.

The doubling periods here :—

78·75—157·5  $\mu$  = 4.39 P.M.—5.17 P.M. = 38 minutes at 24·25—24·75° C.

157·5—315·0 „ = 5.17 „ —5.58 „ = 41 „ 24·75—25·2° C.

Reckoned back :—

423·0—211·5  $\mu$  = 5.34 P.M.—6.17 P.M. = 43 minutes at 25·0—25·5° C.

211·5—105·25 „ = 4.55 „ —5.34 „ = 39 „ 24·6—25·0° C.

Normal doubling periods—

115—230  $\mu$  = 5.0 P.M.—5.40 P.M. = 40 minutes at 24·6—25·0° C.

230—460 „ = 5.40 „ —

not completed, but at present rate not far from 6.24 P.M., which = 44 minutes.

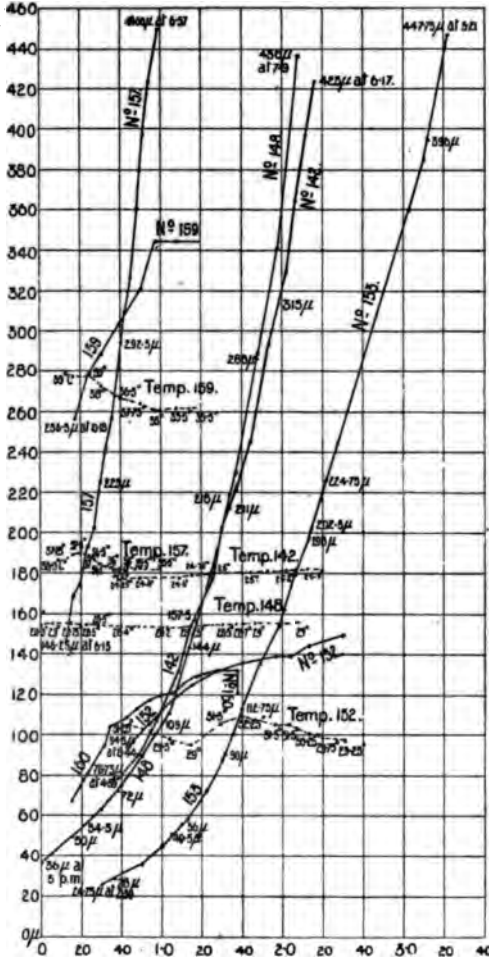
On November 13 a sowing, as before, made at 2 P.M., was put in at 9 P.M., and measured as follows :—

Doubling period :—

58·5—117·0  $\mu$  = 9.3 P.M.—9.50 P.M. = 47 minutes at 24·5—25·75—24·9° C.

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Reckoned back :—

139.5—69.75  $\mu$  = 9.13 P.M.—10.0 P.M. = 47 mins. at 25.75—25.0—24.9—25.0° C.

On November 13 spores, as before, were sown, and put at 22° C. at 12.30 and into the Sachs' box at 6 P.M.

Doubling period :—

36—72  $\mu$  = 6.0 P.M.—6.45 P.M. = 45 minutes at 26.0—26.25° C.

Reckoned back :—

92.25—46.0  $\mu$  = 6.15 P.M.—7.0 P.M. = 45 minutes at 26.1—26.5° C.

Another filament was taken at 8 P.M. and measured as follows :—  
Doubling period :—

225—450  $\mu$  = 8.13 P.M.—8.55 P.M. = 42 minutes at 25.4—25.9° C.

Reckoned back :—

486—243  $\mu$  = 8.17 P.M.—8.59 P.M. = 42 minutes at 25.4—26.0° C.

The following case is a very interesting one. It will be noticed that a drop of KHO was put into the arm of the cell. I was experimenting at the time to see if I could detect any difference by removing what traces of CO<sub>2</sub> diffuse into the cell as fast as they form, but the results were negative in that connection. Incidentally, however, it turned out that the drop of strong KHO often caused a slight dry skin to form on the gelatine drop when first put into the higher temperature, the skin being visible by faint wrinkles in it, and this affects the growth of the organism, possibly by impeding oxygen access. After the drop had melted, all went on quite normally, as we see, and there is no reason to reject the curve as an illustrative one.

On November 10 spores were sown at 10.30 in 10 per cent. gelatine and put into 22° C. A drop of KHO was put in the arm of the cell. At 3.35 the culture was put into Sachs' box at 27.5° C. The opened box fell to 25° C., but recovered to 27° C. at 3.45.

The filament selected did not grow very straight at first, but straightened out later and did well; it was also abnormal in some way, having one joint swollen, but recovered eventually, and after three-quarters of an hour or so behaved quite normally, and the question arose (answered above), had the KHO any effect?

The doubling periods here :—

46—92  $\mu$  = 4.0 P.M.—4.52 P.M. = 52 minutes at 27.2—27.0—27.5° C.

92—184 „ = 4.52 „ —5.36 „ = 44 „ 27.5—27.9—27.75° C.

Reckoned back :—

243.0—121.5  $\mu$  = 5.11 P.M.—5.53 P.M. = 42 m. at 27.75—27.9—27.75—28—27.6° C.  
121.5—60.75 „ = 4.33 „ —5.11 „ = 38 „ 27.0—27.9° C.

Normal doubling periods :—

48—96  $\mu$  = 4.5 P.M.—4.55 P.M. = 50 minutes at 27.0—27.5° C.

96—192 „ = 4.55 „ —5.40 „ = 45 „ 27.5—27.9° C.

I regard the first period of growth as unmeasurable and abnormal, due to something wrong with the growth and position of the filament, but after the first hour it seemed quite normal, as said.

A second filament from the same culture was then examined, as follows :—

The doubling periods here were as follows :—

31·5—63  $\mu$  = 6.5 P.M.—6.40 P.M. = 35 minutes at 29·0—27·2° C.  
 63·0—126 „ = 6.40 „ —7.20 „ = 40 „ 27·2—27·9° C.  
 126·0—252 „ = 7.20 „ —8.11 „ = 51 „ 27·9—27·5° C.

Or, reckoned back :—

319·5 —159·75  $\mu$  = 7.42 P.M.—8.26 P.M. = 44 mins. at 27·75—27·5° C.  
 159·75—79·8 „ = 6.53 „ —7.42 „ = 49 „ 27·25—27·9—27·75° C.  
 79·8 —39·9 „ = 6.16 „ —6.53 „ = 37 „ 28·0—27·2—27·5° C.

A scrutiny of these two tables does not bring out any increase generally in the doubling period during the third to fifth hours of exposure to these temperatures, as compared with the first hour; but the second table suggests that each doubling takes a slightly longer time to effect. It may be that the temperature is not sufficiently far from the optimum to show any great slowing effect in such short periods.

Next morning there was a very good crop in the hanging drop, left at 26·5—27·5° all night, and this supports the above suggestions.

On November 14 spores were sown as before at 11 A.M., and remained at 22° C. till 4 P.M., then into box at 29° C.

The first attempts to fix a filament failed, from floating. Meanwhile the temperature varied as follows :—

| Time.    | Temp.<br>° C. |
|----------|---------------|
| 4.1 P.M. | = 28·25       |
| 4.15 „   | = 29·5        |
| 4.32 „   | = 29·1        |
| 4.45 „   | = 28·75       |

and at 5 P.M. I succeeded in fixing a filament, measured as follows :—

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp. |
|----------|---------|-----------|---------|---------|-------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 5.0 P.M. | 36·0    | —         | —       | —       | 28·9  |
| 5.6 „    | —       | —         | —       | —       | 29·0  |
| 5.15 „   | 49·5    | 15        | 13·5    | 0·9     | 28·9  |
| 5.24 „   | —       | —         | —       | —       | 28·75 |
| 5.29 „   | 63·0    | 14        | 13·5    | 1·0     | 28·5  |
| 5.40 „   | 74·25   | 11        | 11·25   | 1·0     | 28·5  |
| 5.48 „   | 90·0    | 8         | 15·75   | 1·9     | 28·4  |
| 6.2 „    | 112·5   | 14        | 22·5    | 1·6     | 28·2  |
| 6.12 „   | 139·5   | 10        | 27·0    | 2·7     | 28·25 |
| 6.26 „   | 184·5   | 14        | 45·0    | 3·2     | 28·5  |
| 6.37 „   | 231·75  | 11        | 47·25   | 4·3     | 28·7  |
| 6.48 „   | 289·0   | 11        | 57·25   | 5·2     | 29·0  |
| 6.58 „   | 341·75  | 10        | 52·75   | 5·2     | 29·0  |
| 7.9 „    | 436·25  | 11        | 94·5    | 8·6     | 29·0  |

Doubling periods:—

36—72  $\mu$  = 5.0 P.M.—5.37 P.M. = 37 minutes at 28.9—29.0—28.5° C.  
 72—144 „ = 5.37 „ —6.13 „ = 36 „ 28.5—28.25° C.  
 144—288 „ = 6.13 „ —6.48 „ = 35 „ 28.25—29.0° C.

Reckoned back:—

436.0—218.0  $\mu$  = 6.34 P.M.—7.9 P.M. = 35 minutes at 28.6—29.0° C.  
 218.0—109.0 „ = 5.58 „ —6.34 „ = 36 „ 28.3—28.2—28.7° C.  
 109.0—54.5 „ = 5.20 „ —5.58 „ = 38 „ 28.8—28.3° C.

Another partial filament was taken at 8.15.

Doubling periods:—

59.5—119  $\mu$  = 8.15 P.M.—8.49 P.M. = 34 minutes at 28—28.25° C.

Reckoned back:—

144—72  $\mu$  = 8.25 P.M.—9.25 P.M. = 60 minutes at 28.25—28.5—28° C.

There can be little doubt that here we have a case of slowing of growth after five hours' exposure to the temperatures given; *i.e.*, the measurements are here of a piece running down from exhaustion.

On December 3 spores were put at 22° at 9 A.M., and into measurement at 2.35, as follows:—

Normal doubling periods:—

116—232  $\mu$  = 3.5 P.M.—3.35 P.M. = 30 minutes at 28.25—28.6—28.5° C.  
 232—464 „ = 3.35 „ —4.6 „ = 31 „ 28.5—31° C.

Doubling periods:—

87.75—175.5  $\mu$  = 2.54 P.M.—3.24 P.M. = 30 minutes at 28.25—28.6—28.5° C.  
 175.5—351.0 „ = 3.24 „ —3.54 „ = 30 „ 28.5—30.5° C.

Calculated back:—

495.0—247.5  $\mu$  = 3.37 P.M.—4.10 P.M. = 33 minutes at 28.5—31.1° C.  
 247.5—123.75 „ = 3.7 „ —3.37 „ = 30 „ 28.25—28.6—28.5° C.

The most interesting feature in this curve was to see how a steady slow rise of temperature did not prevent a second doubling at nearly the maximum speed. I was not able to measure any further, but saw enough to convince me that the organism was breaking down soon after from exhaustion.

On November 7 spores were sown in normal 10 per cent. gelatine at 1 P.M., and put into incubator at 22° C. At 5.30 the germinated rodlets went into the Sachs' box at 30° C., and at 5.54 the measurements began as follows:—

Doubling period (nearly normal):—

29.25—58.5  $\mu$  = 5.54 P.M.—6.23 P.M. = 29 minutes at 29.5—29—32.6° C.

Further examination of this culture brought out very clearly the difficulties of these high temperatures. I started measurements at 6.28, 7.58, and 8.23 p.m. respectively, but each had to be abandoned after less than a quarter of an hour, owing to the excessive dilution, due to the powerful absorption of water by the (melted) gelatine drop, which induced currents and floated the rodlets away.

The general result was that the longer the culture remained at the high temperature the slower the growth. No doubt the dilution was largely responsible for this, but another cause may have been the slowing action of the high temperatures, taking time to act on the filaments.

It is by no means improbable, comparing all one has observed of these cultures at high temperatures, that the bacterium goes through a grand period of growth, i.e., it begins growing slowly, attains a maximum, and then slows down again until fully outgrown. This being so, the total length of bacterium obtained (which, in other words, represents its total crop, or dry weight) will depend on the following factors:—

1. The amount of available food material. This I cannot measure, because it is not necessarily the amount of *broth*, or *gelatine*, &c., but the amount of peptonised materials, &c., it can get from the medium.

2. The temperature. At the optimum it metabolises, and grows, and respire, &c., at its best; but at higher temperatures removed from that it may grow *for a short time more rapidly*, but sooner exhausts itself, and so produces a poorer crop in the end. This certainly seems to be the case with these high temperature cultures, for I never get anything like so large a crop at 30—35° C. as at 22—25° C., other things being equal. Moreover, it seems clear that though the growth may for a short time be as rapid as it is near the optimum, it soon slows down (*e.g.*, compare November 5 experiments). All this suggests that at temperatures above the optimum, the machinery of the cell is being worked at too high a speed, and comes to grief sooner or later. How far this is due simply to over-respiration and rapid exhaustion of the oxygen in the medium, or to inability to peptonise or digest the food-materials, or other causes, it is impossible to say with certainty. Probably the matter is extremely complex, however.

3. The amount of water in the medium. I have repeatedly pointed out my suspicion that variations in the growth result from variations in the precipitation or absorption of water in the culture-cells. It is not easy to trace the effects to mere dilution of the food materials, however, and it is quite possible that in all cases slight, but perceptible, changes of temperature accompany these evaporations and precipitations, &c. In some cases, at any rate, the dilution is traced to precipitations due to slight draughts, causing

cooling of the cover-slips, so that here, at any rate, the effect is at least in part due to temperature.

These reflections seem justified by the following measurements in the culture last referred to :—

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 8.44 P.M. | 94.5    | —         | —       | —       | 31.25 |
| 8.54 "    | 105.75  | 10        | 15.25   | 1.5     | 30.5  |
| 9.5 "     | 117.0   | 11        | 11.25   | 1.1     | 29.5  |
| 9.15 "    | 126.0   | 10        | 9.0     | 0.9     | 29.0  |
| 9.28 "    | 132.75  | 13        | 6.75    | 0.5     | 31.5  |
| 9.40 "    | 135.0   | 12        | 2.25    | 0.19    | 32.25 |
| 9.54 "    | 139.5   | 14        | 4.5     | 0.3     | 31.5  |
| 10.4 "    | 139.5   | 10        | 0       | 0       | 31.5  |
| 10.13 "   | 144.0   | 9         | 4.5     | 0.5     | 30.25 |
| 10.20 "   | 146.5   | 7         | 2.5     | 0.36    | 29.75 |
| 10.30 "   | 149.0   | 10        | 2.5     | 0.25    | 29.25 |

Curve 152, p. 442.

We see that the growth had become very much slower, and at last had all but ceased. At 8.30 next morning the short crop had begun to try to form a few feeble spores; two control cultures, at 22° C. all the time, had grown more evenly, and developed a crop probably 100 times as big, and had not yet passed over into the spore stage, though they did so during the succeeding forty-eight hours, and then represented a far larger crop, with much more numerous and well-developed spores.

Further criticism of these November 7 experiments confirms the conclusions already arrived at. Thus, during the first hour at 29.5, rising to 32.6° C., and therefore at the large range of 3.6° C. the doubling nevertheless only took twenty-nine minutes; whereas after three hours at similar temperatures, 29.0—32.25° (a range of 3.25) it had not anything like doubled in *two hours*, i.e., the time of exposure to this range tells by slowing the doubling period.

On December 2 spores were put in, at 22°, normal gelatine at 9 A.M., and into measurement at 2.30 P.M., as follows :—


| Time.     | Length. | Interval. | Growth. | Rate.   | Temp.         |
|-----------|---------|-----------|---------|---------|---------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 2.30 P.M. | 24.75   | —         | —       | —       | 30.75         |
| 2.35 "    | —       | —         | —       | —       | 29.5          |
| 2.40 "    | —       | —         | —       | —       | 29.75         |
| 2.45 "    | —       | —         | —       | —       | 30.0          |
| 2.50 "    | 36.0    | 20        | 11.25   | 0.56    | 30.1          |
| 3.0 "     | 45.0    | 10        | 9.0     | 0.9     | 30.2          |
| 3.13 "    | 58.5    | 13        | 13.5    | 1.0     | 30.5          |
| 3.22 "    | 72.0    | 9         | 13.5    | 1.5     | 30.8          |
| 3.30 "    | 87.75   | 8         | 15.75   | 1.9     | 31.0          |
| 3.40 "    | 112.5   | 10        | 24.75   | 2.4     | 31.0          |
| 3.48 "    | 135.0   | 8         | 22.5    | 2.8     | 31.0          |
| 3.58 "    | 158.0   | 10        | 23.0    | 2.3     | 31.2          |
| 4.6 "     | 180.0   | 8         | 22.0    | 2.8     | 31.25         |
| 4.15 "    | 202.5   | 9         | 22.5    | 2.5     | 31.75         |

Curve 153, p. 442.

Normal doubling periods:—

|             |        |                |              |                             |
|-------------|--------|----------------|--------------|-----------------------------|
| 45—90 $\mu$ | = 3.0  | P.M.—3.31 P.M. | = 30 minutes | at 30.2 —31.0 $^{\circ}$ C. |
| 90—180 "    | = 3.31 | " —4.6 "       | = 35 "       | 31.0 —31.25 $^{\circ}$ C.   |
| 180—360 "   | = 4.6  | " —5.3 "       | = 57 "       | 31.25—32.9 $^{\circ}$ C.    |

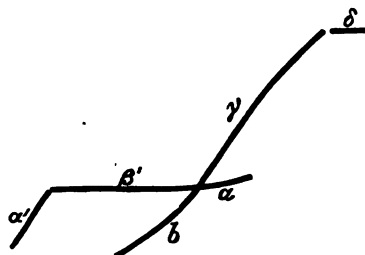
This filament was then left for over half an hour, the temperature continuing the slow but steady rise already seen to have set in, and on resuming the observations it had broken up in a manner very characteristic of such cultures. Since I have not as yet described any of these cases—which occur quite commonly, however—I propose to enter into the details of this one, as an instructive and typical example.

When the filament was measured at 4.6 it showed a slight angular bend near the middle, like this , the segment *a*

being 85.5  $\mu$  long and *b* 94.5  $\mu$  long—total length 180, as registered in the table; when left at 4.15 *a* had grown to 95  $\mu$  and *b* to 107.5  $\mu$ —total 202.5  $\mu$  as seen.

On resuming the measurements at 4.50 several interesting changes in length and position were observable. The segments *a* and *b* had separated, and grown across one another as seen in the accompanying diagram.

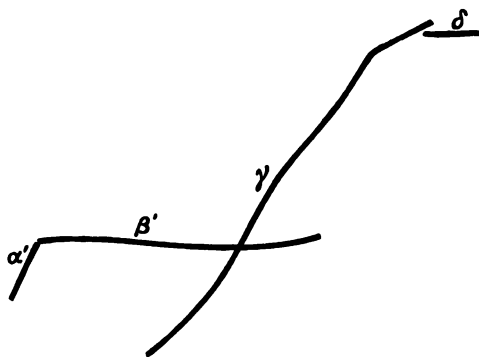
Not only so, but *a* had bent at nearly a right angle into two segments—a short one, *a'*, and a long one,  *$\beta'$* —measuring 45  $\mu$  and 101.25  $\mu$  respectively, while *b* had similarly segmented into a long portion,  *$\gamma$* , measuring 153  $\mu$ , and a short one,  *$\delta$* , measuring 22.5  $\mu$  altogether, but really consisting of a piece of empty sheath—not represented in the diagram—and a piece of solid and normal-looking filament, the latter measured 14  $\mu$ , and the connecting



piece of empty sheath  $8.5 \mu$ . In fact, the disjointing segment  $\delta$  had contracted away in its sheath from the parent filament  $\gamma$ . During the next five minutes the bit of sheath disappeared, and the rodlet began growing again as an independent segment.

Now, it is perfectly feasible to measure all these portions of the original filament, only, of course, it takes two or three minutes to accomplish the necessary movements of the culture—to bring the various segments at angles under the scale—and to turn the eyepiece and read off the measurements. Consequently, one has to record the *mean* time of measurement, and a corresponding slight lack of accuracy results in the curves.

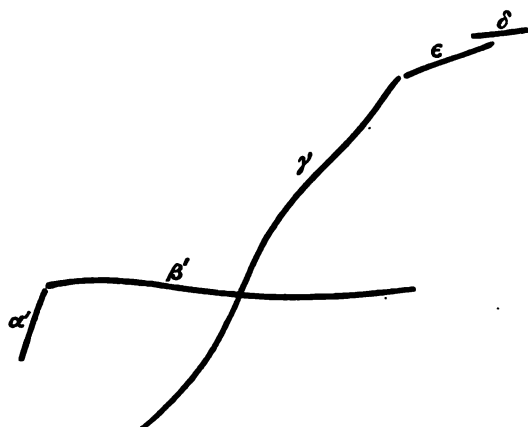
At 5.1, i.e., eleven minutes after the last record, the various segments were grouped as in the following diagram:—



And measured as follows:—

|           |               |              |            |
|-----------|---------------|--------------|------------|
| $\alpha'$ | $= 48.25 \mu$ | having grown | $3.25 \mu$ |
| $\beta'$  | $= 108.0 \mu$ | "            | $6.75 \mu$ |
| $\gamma$  | $= 180.0 \mu$ | "            | $27.0 \mu$ |
| $\delta$  | $= 18.0 \mu$  | "            | $4.0 \mu$  |
| Total..   | $354.25 \mu$  | "            | $41.0 \mu$ |

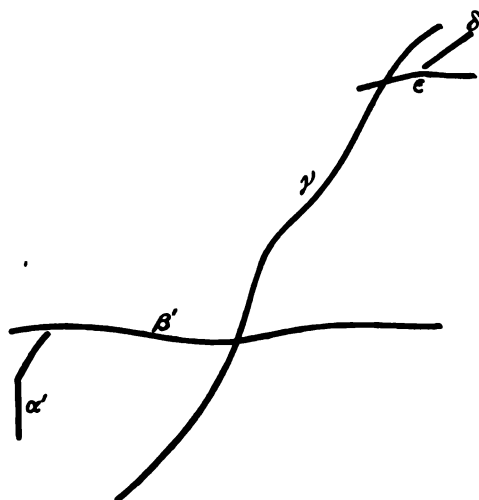
The mean time of the next record was 5.10, and the position was as in the annexed diagram:—



The first change of importance was that a segment,  $\epsilon$ , already bent at an obtuse angle with  $\gamma$  in the last stage, had now separated from  $\gamma$  as an independent rod, the rest (and it) had grown as follows:—

|                       |              |              |
|-----------------------|--------------|--------------|
| $\alpha' = 49.5 \mu$  | having grown | $1.25 \mu$   |
| $\beta' = 112.5 \mu$  | " "          | $4.5 \mu$    |
| $\gamma = 166.5 \mu$  | } " "        | $22.5 \mu$   |
| $\epsilon = 36.0 \mu$ |              |              |
| $\delta = 20.25 \mu$  | " "          | $2.25 \mu$   |
| Total..               |              | $384.75 \mu$ |
|                       |              | $30.5 \mu$   |

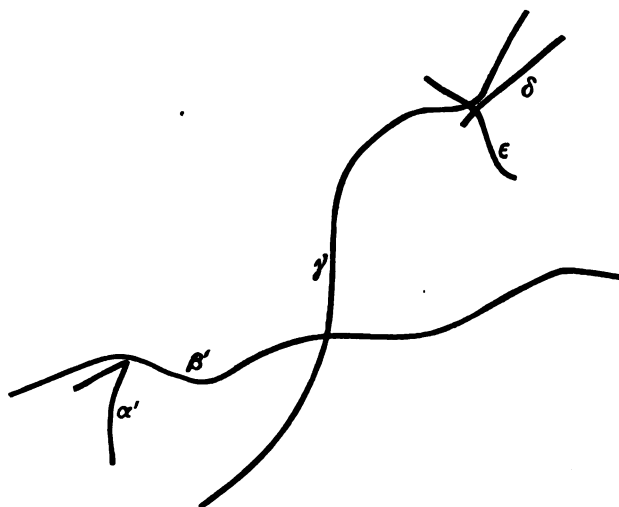
In the next phase the difficulties began to increase. The mean time of measuring was 5.21, and the change of positions and growths are seen from the following diagram and table:—



The changes in position are partly due to slow floating and partly to growth, be it noted.

|            |   |              |              |            |
|------------|---|--------------|--------------|------------|
| $\alpha'$  | = | 56.25 $\mu$  | having grown | 6.75 $\mu$ |
| $\beta'$   | = | 121.5 $\mu$  | " "          | 9.0 $\mu$  |
| $\gamma$   | = | 202.5 $\mu$  | " "          | 36.0 $\mu$ |
| $\epsilon$ | = | 40.5 $\mu$   | " "          | 4.5 $\mu$  |
| $\delta$   | = | 27.0 $\mu$   | " "          | 6.75 $\mu$ |
| Total..    |   | 447.75 $\mu$ | " "          | 63.0 $\mu$ |

At 5.30, as near as could be judged, the diagram of positions stood thus :—



But it was no longer possible to obtain measurements on which any reliance could be placed, and so I abandoned the culture.

It is interesting at least to put the foregoing totals together as a continuation of the measurements made when the filaments were easily dealt with :—

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp.         |
|-----------|---------|-----------|---------|---------|---------------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | $^{\circ}$ C. |
| 4.50 P.M. | 313.25  | 35        | 110.75  | 3.16    | 32.25         |
| 5.1 "     | 354.25  | 11        | 41.0    | 3.6     | 32.5          |
| 5.10 "    | 384.75  | 9         | 30.5    | 3.4     | 32.75         |
| 5.21 "    | 447.75  | 11        | 63.0    | 5.7     | 32.9          |

Curve 153 (continued).

The doubling periods (of entire curve) :—

|                  |                      |              |                         |
|------------------|----------------------|--------------|-------------------------|
| 24.75—49.5 $\mu$ | = 2.30 P.M.—3.4 P.M. | = 34 minutes | at 30.75—29.5—30.25° C. |
| 49.5 — 99.0 „    | = 3.4 „ —3.35 „      | = 31 „       | 30.25—31° C.            |
| 99.0 —198.0 „    | = 3.35 „ —4.13 „     | = 38 „       | 31—31.75° C.            |
| 198.0 —396.0 „   | = 4.13 „ —5.12 „     | = 62 „       | 31.75—32.75° C.         |

Calculated back :—

|                     |                       |              |                   |
|---------------------|-----------------------|--------------|-------------------|
| 447.75—223.75 $\mu$ | = 4.22 P.M.—5.21 P.M. | = 59 minutes | at 31.75—32.9° C. |
| 112.0 —224.0 „      | = 3.40 „ —4.22 „      | = 42 „       | 31—31.75° C.      |
| 56.0 —112.0 „       | = 3.10 „ —3.40 „      | = 30 „       | 30.25—31° C.      |
| 28.0 —56.0 „        | = 2.36 „ —3.10 „      | = 34 „       | 29.5—30.25° C.    |

It is evident that even if we allow a wide margin for errors, the curve and tables show that the period of breaking up of the filament coincides with a period of slowing of the growth, *i.e.*, the high temperature was beginning to tell on the growth, and the fourth doubling period was nearly twice as long as the preceding ones.

This accords very well with the experiment of November 7, where the first doubling period was twenty-nine minutes, but that of a later rod in the same culture was found to be over 120 minutes, fully bearing out the conclusions there arrived at, namely, that the temperature begins to tell after a longer or shorter time.

This culture had broken up into short separate segments of two to five cells each at 10 P.M. (*t.* = 31.5), and on December 3 at 9 A.M., had formed a few feeble spores here and there, while many empty bits of sheath were seen. The sister culture kept at 22° all the time was now a fine normal crop of long-coiled tresses, and must have measured several thousand times the lengths of the starved-looking crop above described. Nevertheless, the drop of gelatine in the 22° C. culture was *certainly no bigger* than that at the higher temperature, but, if anything, a little smaller. These cultures alone were sufficient to convince me that it is not a question of quantity of food-materials originally given.

On November 5 spores were sown at 9 A.M. in 10 per cent. normal gelatine, and kept at 22° C. until 3.10, when the germinated filaments were put into the Sachs' box, which stood at 35° C.

A filament was selected measuring 40.5  $\mu$  with the following results. It must be remembered that this first series of measurements was made during the period of accommodation to the high temperature, and while the box—necessarily opened for arrangement—was recovering from the consequent lowering of the temperature. Variations of temperature are much more difficult to control as they get higher.

The filaments in this normal gelatine show their septa much more sharply than in broth, and, as we see, grow fairly rapidly at these temperatures.

The curve shows that the doubling period here was :—

Normal doubling period :—

58·5—117·0  $\mu$  = 3.40 P.M.—4.9 P.M. = 29 minutes at 35·5—33·75° C.  
40·5—81·0 „ = 3.20 „ —3.55 „ = 35 „ 32—35·5—34·3° C.

Calculated back :—

141·75—70·8  $\mu$  = 3.48 P.M.—4.15 P.M. = 27 minutes at 34·5—33·25° C.

*I.e.*, with a range of 3·5° C. it took 35 minutes to double, whereas with a range of 1·25° C. it only took 27 minutes.

But we must scrutinise cautiously these results with varying high temperatures acting on a culture in process of accommodation.

At 4.28 another filament was selected, and this was showing the slight but distinct writhing movements so often seen at high temperatures. The movement was especially noticeable in a segment, about 20  $\mu$  long, which was slowly bending backwards and forwards, and which had broken off, when 22·5  $\mu$  long, at 4.50: since it remained close to the parent segment, however, it was easily measurable with it.

This had to be abandoned now, the measurements becoming too difficult. The rate of growth was fairly rapid, and the length was doubled as follows :—

126—252  $\mu$  = 4.28 P.M.—5.4 P.M. = 36 minutes at 34·75—35—34—35° C.

Which—taken with the preceding—shows that strong plants, once germinated out, can stand these high temperatures (even varying) very well for a time, though their growth is correspondingly irregular.

This last point is even more evident from the following, however. The culture was left at 35°—varying a little to either side—till 8 P.M., and another rodlet selected and measured, as follows :—

And on analysing the doubling periods we find a considerable retardation, evidently due to the variations in growth at the high temperatures.

Doubling periods :—

67·5—135  $\mu$  = 8.19 P.M.—9.13 P.M. = 54 minutes at 35·4—34·25—37·5—35·5° C.

Calculated back :—

166·5—83·25  $\mu$  = 8.35 P.M.—9.31 P.M. = 56 m. at 34·25—37·5—35·25—37·75° C.

It should be noted that the drop became copiously diluted after an hour or so, by absorbing water, and the longer doubling period (36 minutes with a range of 1° C.) from 4.28 onwards is possibly referable to this rather than the age of the rodlet; though I think the latter fact of importance, as we shall see.

On examining these curves of November 5—all from one culture be it remembered—we find that when first put in the doubling took 35 minutes, at 32—35·5° a range of 3·5°; whereas the part growing at 33·25—34·5° (a range of 1·5°) only took 27 minutes, whence may

be inferred that the slowing was due to either (1) lack of accommodation, or (2) the greater variation of temperature.

Then we find, after  $1\frac{1}{2}$  hours at the temperatures given, the doubling took 36 minutes at 34 to 35°—a range of 1° C. only—which certainly suggests that it is the temperature, and *not the range* which is effective in slowing the growth.

Next—same culture, but having now been 5 hours at these temperatures—we find 54 and 56 minutes respectively necessary to double the length at 34.25 to 37.75° C., a range of 3.5°; and since the same range was experienced by the first filament mentioned, but which was measured during the first hour or so of exposure to these temperatures, we can no longer doubt that the slowing is due to the gradual effect of the temperature, and *not to the range*.

This means that at the temperatures given the rate of growth gradually slows, and the grand curve takes a shorter shape, and, on consulting the previous series of curves, the same fact becomes apparent.

On November 9 spores were sown at 11.30 A.M. in 10 per cent. gelatine, and put in at 22° C.; at 6 P.M. they had germinated to filaments over 100  $\mu$  long, and the culture was put into Sachs' box at 36—37° C.

In order to see if any body, such as CO<sub>2</sub>, caused or hastened the running down of the culture, I put a little KHO solution in one arm of the cell, next the cotton-wool plug.\* This was suggested by the experiments of November 7, and previous observations on the extraordinary irregularities and slowing of the growth at these high temperatures.

The measurements were as follows:—

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 6.13 P.M. | 146.25  | —         | —       | —       | 36.5  |
| 6.17 "    | 168.75  | 4         | 22.5    | 5.6     | 37.25 |
| 6.20 "    | 173.25  | 3         | 4.5     | 1.5     | 37.5  |
| 6.24 "    | 193.5   | 4         | 20.25   | 5.6     | 37.5  |
| 6.27 "    | 202.5   | 3         | 9.0     | 3.0     | 37.0  |
| 6.30 "    | 225.0   | 3         | 22.5    | 7.5     | 36.5  |
| 6.33 "    | 243.0   | 3         | 18.0    | 6.0     | 36.25 |
| 6.36 "    | 256.5   | 3         | 13.5    | 4.5     | 36.1  |
| 6.39 "    | 281.25  | 3         | 24.75   | 8.2     | 36.0  |
| 6.42 "    | 308.25  | 3         | 27.0    | 9.0     | 35.75 |
| 6.45 "    | 326.25  | 3         | 18.0    | 6.0     | 36.0  |
| 6.48 "    | 360.0   | 3         | 33.75   | 11.2    | 35.5  |
| 6.51 "    | 396.0   | 3         | 36.0    | 12.0    | 35.5  |
| 6.54 "    | 423.0   | 3         | 27.0    | 9.0     | 35.5  |
| 6.57 "    | 450.0   | 3         | 27.0    | 9.0     | 35.5  |

Curve 157, p. 442.

\* See also experiments on p. 443.

The filament then broke quite suddenly, shooting off a short piece, which I measured by itself (see below).

Doubling periods :—

146·25—292·5  $\mu$  = 6.13 P.M.—6.40 P.M. = 27 minutes at 36·5—37·5—36° C.

Reckoned back :—

225—450  $\mu$  = 6.30 P.M.—6.57 P.M. = 27 minutes at 36·5—35·75—36—35·5° C.,

and this is also the normal doubling period.

The piece which broke off was measured as follows :—

| Time.    | Length. | Interval. | Growth. | Rate.   | Temp. |
|----------|---------|-----------|---------|---------|-------|
|          | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 7.0 P.M. | 63·0    | —         | —       | —       | 35·9  |
| 7.5 "    | 72·0    | 5         | 9·0     | 1·8     | 35·0  |
| 7.10 "   | 81·0    | 5         | 9·0     | 1·8     | 35·0  |
| 7.15 "   | 90·0    | 5         | 9·0     | 1·8     | 35·0  |
| 7.20 "   | 100·0   | 5         | 10·0    | 2·0     | 35·0  |
| 7.25 "   | 113·5   | 5         | 13·5    | 2·7     | 35·75 |
| 7.30 "   | 122·5   | 5         | 9·0     | 1·8     | 35·6  |
| 7.35 "   | 140·5   | 5         | 18·0    | 3·6     | 35·5  |
| 7.40 "   | 158·5   | 5         | 18·0    | 3·6     | 35·0  |

Doubling periods :—

63—126  $\mu$  = 7 P.M.—7.31 P.M. = 31 minutes at 35·9—35—35·75—35·6° C.

Reckoned back :—

158·5—79  $\mu$  = 7.3 P.M.—7.40 P.M. = 31 minutes at 35—35·75—35° C.

On resuming the measurements at 8.30, I found considerable growth still going on, but the filaments floated about so in the now diluted gelatine, that it was not till after 10 P.M. that I succeeded in fixing one, as follows. Moreover, the temperature had risen considerably (to 39° C.), a fact which makes the following measurements only the more interesting.

| Time.      | Length. | Interval. | Growth. | Rate.   | Temp. |
|------------|---------|-----------|---------|---------|-------|
|            | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 10.18 A.M. | 256·5   | —         | —       | —       | 39·0  |
| 10.24 "    | 279·0   | 6         | 22·5    | 3·7     | 39·0  |
| 10.30 "    | 288·0   | 6         | 9·0     | 1·5     | 38·0  |
| 10.40 "    | 303·75  | 10        | 15·75   | 1·5     | 36·5  |
| 10.50 "    | 321·75  | 10        | 18·0    | 1·8     | 35·75 |
| 11.0 P.M.  | 344·25  | 10        | 22·5    | 2·2     | 35·0  |
| 11.10 "    | 345·0   | 10        | 0·75    | 0·07    | 35·5  |
| 11.20 "    | 345·0   | 10        | 0·0     | 0·0     | 35·5  |

The filament had gradually acquired the granulated appearance which comes on when dying.

It will be noted that this curve is more depressed than the preceding, and that it has nothing like doubled itself in the hour; still, it is, perhaps, remarkable that growth occurred at all.

Here, again, we find that during the first hour of exposure to *even higher* temperatures than on November 5 and 7, it only took twenty-seven minutes to double the length at 36—37·5° C., a range of 1·5° C. (*cf.* November 5, at a range of 1·25°), or at 35·5° to 36·5° (a range of 1° C.), whereas during the second hour of exposure it took thirty-one minutes to double, though the range of temperature was only 0·75° to 0·9° C., *i.e.*, from 35—35·9° and from 35—35·75° C.

The matter is rendered the more interesting by what occurred during the fifth hour of exposure (10.18 to 11.20 P.M.), for we find that in sixty-two minutes the filament had not accomplished even half the doubling, but had slowly ceased to grow at all.

It is true, this last case taken alone is open to the objection that the temperature rose so high (35·5—39°), but, taken in conjunction with the experience of November 5 and 7, I think it is important.

Moreover, this experiment of November 9 does not support the suggestion that the gradual slowing is due to the accumulation of CO<sub>2</sub>, or any volatile body inhibitive of the growth and absorbable by KHO. Of course it does not disprove any such hypothesis, but it shows that the slowing is not a *mere* matter of CO<sub>2</sub> accumulation, and (considering the capacity of my cells and all the conditions) it can hardly be a mere starvation of oxygen supply.

The results point to intense destructive metabolism, possibly respiratory, as the cause of death, and suggests that the high temperatures over-work the machinery of the cells, and, no doubt, the irregular, jerky growth shown on the curves is an expression of this. At the same time, it is not impossible that the highly stimulated organism is here so extremely sensitive to minute changes in the environment that these sudden variations are, in part, due to alterations not recorded.

On December 3 spores sown at 2 P.M., normal gelatine at 22° C., were ready at 7.50.

| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 8.12 P.M. | 153     | —         | —       | —       | 38·0  |
| 8.24 "    | 171     | 12        | 18      | 1·5     | 38·0  |
| 8.34 "    | 180     | 10        | 9       | 0·9     | 37·9  |
| 8.49 "    | 225     | 15        | 45      | 3·0     | 38·5  |

At 8.34 the filament had curved badly, and began breaking up, and at 8.49 it was broken into several writhing curved segments, difficult to measure. At 8.51 all growth had ceased, and by 9.5 the broken fragments were becoming granular, the temperature meanwhile having gone up to over 39° C.

On December 3 spores sown at 9 A.M., normal gelatine, at 22° C., were put into measurement at temperatures near 39–40° C. at 9.15 P.M. On opening the Sachs' box the temperature fell from 39.75° to 35°, and had to work up again with some difficulty, as the opening of the side windows, &c., has more effect at these high temperatures than at lower ones.

The results are as follows :—

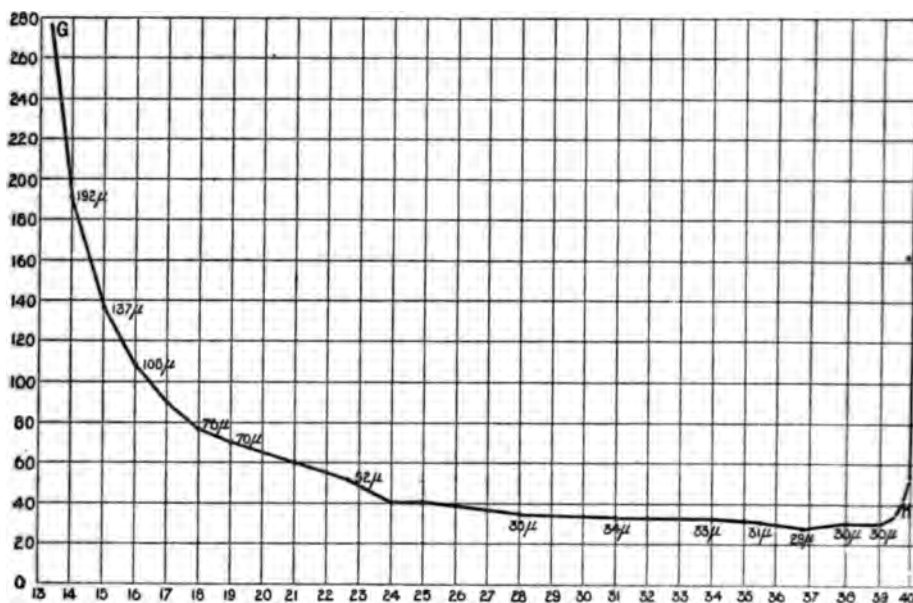
| Time.     | Length. | Interval. | Growth. | Rate.   | Temp. |
|-----------|---------|-----------|---------|---------|-------|
|           | $\mu$ . | mins.     | $\mu$ . | $\mu$ . | ° C.  |
| 9.15 P.M. | 65.5    | —         | —       | —       | 35.0  |
| 9.23 "    | 81.0    | 8         | 13.5    | 1.7     | 37.0  |
| 9.32 "    | 91.5    | 9         | 13.5    | 1.5     | 37.75 |
| 9.35 "    | 103.5   | 3         | 9.0     | 3.0     | 38.75 |
| 9.42 "    | 108.0   | 7         | 4.5     | 0.6     | 40.0  |
| 9.47 "    | 112.5   | 5         | 4.5     | 0.9     | 41.0  |
| 9.55 "    | 117.0   | 8         | 4.5     | 0.6     | 41.5  |
| 10.0 "    | 119.25  | 5         | 2.25    | 0.4     | 41.3  |
| 10.5 "    | 121.5   | 5         | 2.25    | 0.4     | 41.25 |
| 10.11 "   | 126.0   | 6         | 4.5     | 0.7     | 40.8  |
| 10.17 "   | 128.25  | 6         | 2.25    | 0.3     | 40.6  |
| 10.22 "   | 130.5   | 5         | 2.25    | 0.4     | 40.6  |
| 10.30 "   | 132.5   | 8         | 2.0     | 0.25    | 41.0  |

Curve 160, p. 442.

At 10.39 the filament, growing no longer, sharply contracted to 117  $\mu$ , and passed over into the granular condition which indicates death.

This case is interesting, because it evidently marks the limit of possible growth of this bacillus as nearly as possible. The specimen was a very vigorous one, and there was no reason why it should not have gone on growing vigorously for some time yet at temperatures near the optimum, and, indeed, we see it began growing at a pretty good rate as the temperature rose to 39–40° from 9.15 to 9.35, and thence the curve depressed more and more, and the doubling period became prolonged to infinity.

In the curve G H, I have summarised the results of all averages of my curves of normal doubling periods obtained in this way. The ordinates are minutes, the abscissæ degrees centigrade.



The ordinates are minutes, the abscissæ temperatures.

### Summary.

We see from various experiments—*e.g.*, those of November 17 and 18—that growth can and does take place as low as  $8.5^{\circ}\text{C}$ ., but so slowly that it is extremely inconvenient to get a curve sufficiently long for my purposes. When we remember that it takes from about 360 to 400 minutes, or even longer, for a filament to double its length at these temperatures, it is obvious that one cannot trace many of them; for even supposing 360 minutes to be the average, it would mean twelve hours' continued attention to get two doubling periods, to say nothing of the six hours or more necessary for germination, so that variations impossible to watch may take place during the long intervals between some of the observations.

Consequently I have but few records of these slow periods at low temperatures near this.

It is interesting to note, however, that  $8.5^{\circ}\text{C}$ . is certainly not the minimum, though it probably approaches it; this is important in connection with the river habitat of the organism.

Some of the experiments show that the growth is still very slow at temperatures near  $10\text{--}12^{\circ}\text{C}$ ., but it is impossible to say more than that the doubling period is here about 300 to 400 minutes or so. The observations begin to be more definite near  $14^{\circ}\text{C}$ ., and we may put the normal doubling period at not far from 200 minutes.

Passing on from this point the doubling periods fall to about 100 minutes near 16° C., and the shape of the curve now begins to be apparent (see Curve G H); about 70 to 80 minutes near 18°, 60 to 70 minutes near 20°, falling to 50 minutes near 22°, and about 40 minutes near 24° C. to 26° C., 35 to 30 minutes near 28—30° C., and this rate of growth may be kept up, for a short time at any rate, at all temperatures thence to 36° or 37° C., but with certain complications to be referred to shortly.

It may be inferred from the facts, that growth occurs, at a *very* slow rate, at some minimum temperature near 5° or 6° C., and that starting just beyond an infinitely long period, the doubling period at the minimum temperature occupies many hundreds of minutes, rapidly shortening for each degree as we raise the temperature to 12—15°, and falling more slowly at each step as the temperature becomes more favourable.

It never seems to fall below about thirty minutes, however (the lowest recorded for a post-germinal filament is twenty-seven minutes), and we may therefore assume that at the quickest rate, cell-division demands thirty minutes or so for its completion—for it will be conceded that the doubling period of a filament is the expression of the time occupied by a complete cell-division and growth, since it is evident from the uniform length of the cells that when the filament is twice as long as at first, it has twice as many cells in it.

Now the experiments show over and over again that at higher temperatures—*i.e.*, temperatures beyond 25—26° C.—this minimum doubling period is only approached during the early stages of the growth at the given temperature, and when we reach temperatures near 30° C. and beyond, the filaments occupy fifty or sixty or even more minutes to complete a second or third doubling, and after a time fail to complete the period if the high temperature is maintained.

In other words, these high temperatures gradually tell on the power of the organism to maintain its best rate of growth, and the doubling period gets longer and longer the further the temperature is beyond the optimum. The optimum temperature thus comes to be that temperature at which the organism can longest maintain its ability to double its length (or, which is the same thing, complete its cell-divisions) in the shortest time, and so turn the maximum amount of food-materials into cell-substance—or, crop. This optimum temperature is 25° C. to 28° C., or very near these, as the curves show.

The maximum temperature, therefore, is not a fixed point, until we approach 39° to 40° C., beyond which no growth seems possible; but it differs according to the length of time the organism has been exposed to the high temperature.

Thus, it frequently occurs that a *first* doubling period is completed

at even 35° or 36° C., in the minimum time—*i.e.*, thirty minutes or so—but the *second* doubling of the same filament will require a longer time, and the *third* may occupy nearly twice as long, and so on, as is well seen from the curves of November 5.

Another way of putting it is the following—the higher the temperature (up to the limit) is above the optimum the *sooner* the total growth of the organism is completed, but as the *rate* of this growth does not increase beyond that of the thirty minutes doubling period, the total length of the filament produced is proportionately less than would have been produced nearer the optimum, *i.e.*, the total crop is a smaller one.

As I understand it, this action of temperature is on the life of the organism, and not a mere exhaustion of the food-medium; and I suppose that in like manner the action of the blue rays of light is similarly on the organism, and not merely on the food-medium.

But it by no means follows that the food medium is totally exempt from oxidation under the action of both temperature and light; and in the case of such extremely oxidisable media as peptone-meat-broth, and similar compounds, *the constitution of which approaches as near to the bodies concerned in metabolism as any media physiologists have been able to prepare*, we must not be surprised to find that they undergo oxidation *outside*, as well as *inside* the living cells, especially when enzymes are present, under the action of light and high temperatures.

Many of the experiments point to this conclusion, and it is particularly to be noticed that the food-medium seems to become more and more subject to such oxidations—promoted by high temperatures or by light—when the action of the peptonising enzymes, which they excrete, is in full swing, as if the whole system—cell *plus* its peptonised medium—of the hanging-drop were respiring, so to speak.

It will, I think, be worth while to institute a careful series of experiments specially directed to secure information on this head; for it opens up a very large and important question.

Elfving's researches\* have, it seems to me, already rendered it extremely probable that, in the case of the fungi he examined, the action of the light is to destroy, by promoting oxidation, the constructed metabolites *at the moment they are about to be assimilated*. It looks, in fact, as if the materials to be built up in the protoplasmic structure are in a dangerously unstable—*explosive*, if you like—condition, and no doubt it is at this period that the damage is done, probably in the machinery of the cell itself, though it is not impossible that it occurs outside the actual machinery, in vacuoles, for instance.

Now peptone-broth, *when saturated with enzymes* of the kind referred to, is doubtless an extremely unstable medium, and probably in a

\* Elfving, "Studien über die Einwirkung des Lichtes auf die Pilze," 1890.

condition more like the one in which it enters into the constitution of the living substance than any other organic substance we know—or at any rate that we can prepare and use outside the living cell.

In a certain sense then, it is possibly not absurd to compare the peptonised broth-drop, in which a bacterium cell is suspended, to a sort of inverted living cell in which the peptonised food medium should normally be *inside* the protoplasmic lining, *i.e.*, in a vacuole. In other words, we might, perhaps, roughly compare it to a vacuolated cell turned inside out, a comparison the more justifiable since the bacterium cell seems to have no obvious vacuole: instead of its assimilable food solution being clothed by the protoplasm, its protoplasm is clothed by the assimilable food-solution (of course I am neglecting the cell-membrane, and do not wish to push the analogy too far) in a highly unstable condition.

The practical aspects of this are also interesting, for it will rarely happen that a bacterium cell, or spore, escapes without organic material clinging to it—a matter of importance even if it were shown that the action of the light was entirely confined to promoting oxidations at the surface of the cells.

Taking all into account, therefore, it might perhaps be worth investigation (if methods can be devised) how far some heliotropic effects and retardations of growth in higher plants, and the retarding action of light on growth generally, are due to *destructive oxidations in the cell-sap of highly combustible food-materials at, or just prior to, the moment they are ready to be assimilated into the living substance of the protoplasm.*

It might possibly remove some of the difficulties connected with the theory of heliotropism of non-cellular cœloblasts, if the light action occurs in vacuoles next the source of illumination—though I confess I see no way out of the difficulties of so-called negative heliotropism, on this or any other hypothesis.

It certainly suggests reasons why so many adaptations occur in nature to protect fluids, which presumably contain such substances as I have referred to, by colour screens of exactly the kind we should expect to be efficient, as I have already referred to elsewhere.

An interesting result follows from the fact that the doubling period is simply the visible expression of the doubling in length and bipartition of all the cells composing the filament.


Suppose a filament to be  $50\ \mu$  long, and composed of ten cells each  $5\ \mu$  long, and that that filament doubles its length in thirty minutes at a given temperature: then the filament, now  $100\ \mu$  long, consists of twenty cells, each of which has taken thirty minutes to divide and double itself; from this we can deduce the number of bacilli formed in a given time from the doubling periods, although the individual bacilli are themselves invisible, and when we find a curve like that of

August 4, where the filament grows from  $10\ \mu$  to  $652\ \mu$  in length, at  $21-23.7^\circ$ , with an average doubling period of about thirty-five minutes, it can be translated as meaning that the number of bacilli increased as follows:—

|     |                         |             |        |   |
|-----|-------------------------|-------------|--------|---|
| 2   | bacilli                 | became      |        |   |
| 4   | at the end of the first | 35 minutes. |        |   |
| 8   | „                       | „           | second | „ |
| 16  | „                       | „           | third  | „ |
| 32  | „                       | „           | fourth | „ |
| 64  | „                       | „           | fifth  | „ |
| 128 | „                       | „           | sixth  | „ |

and so on, and we may assume that if the supply of food-material could be kept constant, and no disturbing conditions set in, this would go on. If it went on for only half a day—twelve hours—there would be nearly 4,000,000 of the bacilli produced from the pair started with above, and the filament would be nearly  $40,000,000\ \mu$  in length—i.e., nearly 40 metres—whence some idea may be obtained of the energy of the growth on the one hand, and of the limits imposed by the culture-drops on the other; for if we take the size of a drop as 1 cubic mm., which is approximately the volume of a hanging drop such as is used in the cultures, and remember that the bacilli in question are about  $1.75\ \mu$  in diameter, it will be found that the above length of 40 metres, nevertheless, has plenty of room in the drop, for the filaments have a volume of only 96,250,000 cubic  $\mu$  to pack away in the 1,000,000,000 cubic  $\mu$  of the drop, so that we see the latter *could* hold ten times the quantity.

We are now in a position to resume the discussion of these growth-curves in detail, and the action of temperature, &c., on this schizomycete, with more hope of success.

It is evident that the normal growth-curve is one which begins to rise slowly, and gradually gets steeper and steeper, and then slowly rises less and less rapidly until the end. This gives a curve like a long drawn out .

At the optimum temperature the growth is very rapid, and lasts for a long time, and the organism uses the materials to maximum effect and produces from them the maximum amount of its own substance—in other words, the largest “crop.”

At temperatures above the optimum, however, the growth, though at first as rapid as at the optimum temperature, lasts for a shorter and shorter time, according as the temperature is further and further removed from the optimum; consequently, the curve, though equally steep in its steepest parts, begins to fall sooner, and growth ceases sooner, and the crop obtained from the same amount of original food-material is smaller and smaller according as the temperature is higher.

At length a temperature is reached where the curve is infinitely short, *i.e.*, no growth occurs at all. This temperature is, however, above 39° C., and indicates the death-point.

Taking temperatures below the optimum. There is a point, somewhere below 8° C., where the curve is indefinitely postponed, *i.e.*, no growth can occur at all. Then comes a temperature, also below 8—10° C., where the curve ascends slowly and never attains the steepness of the curve at optimum temperature. This is the minimum temperature.

At temperatures above the minimum the curve attains more and more nearly, and in shorter and shorter times, to the steepness of the normal curve, the nearer the temperature in question is to the optimum temperature.

This optimum temperature is either 25° C. or some point very near it.

The above case of the normal curve is the hypothetical one where *all* the conditions are constant, a state of affairs never realised.\* During the growth, between the period when the germination is completed and the organism no longer obtains any supplies from the spore, but is totally dependent on the food-materials given it, and the period when the curve begins to ascend less rapidly, there is a period of maximum growth, during which the filament doubles its length in equal minimum times. This is the critical period of the curve. The more closely the curve approximates to the normal curve the longer this phase of equal minimum doubling periods lasts; the more external conditions affect the curve the shorter this phase is, and the longer the doubling periods become.

The factors affecting the curve may be regarded as of two kinds, internal and external, though they probably never vary entirely independently.

The internal factors are such as (1) irregularities of cell-divisions: if a single cell fails to divide in due order, the curve is at once affected, because the regularity of the intercalary growth of the filament is destroyed, and this occasionally happens. (2) The separation of the segments: several observations suggest that the growth is slowed at once when the new surfaces of the broken ends come in contact with the food-medium. (3) Nutations and oscillatory movements, though possibly these affect the *measurements* rather than actual growth. (4) Unknown internal factors which affect the rapidity of germination, the ability to assimilate the food-materials, and so forth. In some cases these may be due to pathological conditions, as in the case given on p. 392.


\* Theoretically, with absolute uniformity of conditions, including food supply, the curve would go on to infinity, and the doubling periods be equal throughout; the fall above would then be indefinitely postponed.

The external factors are: (1) temperature. Variations in the curve are produced by sudden variations in the temperature, and, apparently, the variations are the more pronounced the *quicker* the temperature changes and the more *extensive* their range; but the amount of variation in the curve, due to any given rise or fall of temperature in constant time, appears to depend on the distance of the temperature (from which the variation is reckoned) from the optimum. In other words, the sensitiveness of the organism to a rise or fall of a degree centigrade varies according to the temperature from which the rise or fall occurs, for, if it has been growing at 30° C., constant temperature, for an hour, it shows a more marked deflection on the curve for a sudden rise or fall of 1° C. than for the same sudden rise or fall from 25° C.

That the variation in rate of growth which has been going on at any hitherto constant temperature is more pronounced when the rise or fall is 2° C. than when it is only 1° C. will be obvious, and similarly for any other range; but, again, it must be noted that the *amount* of deflection of the curve for *any* range of variation depends on the mean temperature, or the hitherto constant temperature, at which the growth has been going on.

The practical importance of all this on experiments on light is very great, and the difficulty, amounting almost to impossibility, of arranging two cultures differently illuminated (if the sunlight is at all intense), so that each shall be growing at exactly the same temperature during the observations, limits the absolute value of the determinations, so far as the curve of growth is concerned.

But there are other points besides (1) the actual temperature when constant, (2) the suddenness of any rise or fall, and (3) the range of temperature, referred to above.

As we have seen, the time during which the organism is exposed to any given (constant) temperature above the optimum affects the growth considerably. The general result of the observations is that the higher the temperature is above the optimum the more rapidly the organism completes its -shaped curve of growth—i.e., the quicker it passes through the phase of maximum rapidity of growth into that of cessation—and consequently the smaller the crop produced from a given amount of food-materials.

For instance, if the spores germinate out at 22° C., and are made to complete their growth at 30° C., the amount of growth (or, in other words, the crop produced) will be much greater if the exposure to 30° is delayed till the *eighth* hour after sowing than if it is made at the *sixth* hour. In both cases the growth at the higher temperature is at maximum rapidity at first, and then rapidly declines. Suppose two cultures at 22° C., the spores having been sown at 12.0 noon: one is put in at 30° at the sixth hour (6 P.M.) after sowing,

when the filaments are  $20\ \mu$  long, the other not until the eighth hour (8 P.M.), when  $320\ \mu$  long. The first will grow at maximum rate, or nearly so, for the first hour, and the filaments double their length twice during that time, therefore =  $80\ \mu$  long (at 7 P.M.), and then begin to decline; the second will also double twice in the hour (*i.e.*, at 9 P.M.), and therefore be  $1280\ \mu$  long when growth commences to decline.

Suppose during the second hour at  $30^\circ\text{C}$ . the doubling period is twice as long as during the first hour, then the first culture will have its filaments  $160\ \mu$  long, and the second one  $2560\ \mu$  long at the finish (8 P.M. and 10 P.M. respectively). If during the third and fourth hours the period is again doubled, the first culture at the end of the fourth hour (10 P.M.) is  $320\ \mu$  long, while the second culture (at 12 P.M.) is  $5120\ \mu$ , and so on.

If both had remained at  $22^\circ\text{C}$ ., and maintained a doubling period of, say, thirty minutes through the whole time regarded above (as experiments show would be more or less the case), then, supposing  $20\ \mu$  to be the length at starting (6 P.M.), it would be  $320\ \mu$  at the end of two hours (8 P.M.), and  $1280\ \mu$  at the end of the next hour (9 P.M.), corresponding to the first hour at  $30^\circ$  above,  $5120\ \mu$  at the end of the next hour (10 P.M.), and  $81,920\ \mu$  after two hours more (*i.e.*, 12 P.M.).

That is to say, at the more favourable temperature of  $22^\circ\text{C}$ .\* the filaments would attain a length of  $81,920\ \mu$  in the same time as they take to reach  $5120\ \mu$ , or even  $320\ \mu$ , if their last four hours or so are spent at the higher unfavourable temperature.

No doubt the ideal case given could not be actually realised, but there can be no question that an approximation to it is what occurs, and the matter resolves itself into this:—At temperatures above the optimum the organism cannot get as much out of the nutrient materials as it can at temperatures near the optimum or below it, evidently because there is some more or less enormous waste of energy expended in doing something which no longer contributes to the nutrition of the protoplasm, and this the more the higher the temperature. To say that the respiration is rendered too intense in proportion to the constructive metabolism is, no doubt, true, so far as it goes; but this does not explain the probably complex matter beyond a certain as yet unsatisfactory point.

There is one aspect of the matter worth noting. I imagine no physiologist would allow that the destructive waste of energy going on here has its seat solely in the food-materials, but would agree that it is in the protoplasm of the cell; this is of importance, because we must conclude that some presumably similar waste of energy goes on

\*  $22^\circ\text{C}$ . is chosen because it was convenient to work with, as being that used; but  $25^\circ\text{C}$ . is nearer the optimum.

at temperatures otherwise suitable for growth when light rays at the blue-violet end of the spectrum act on the growing cells or the spores.

Unless it can be shown that the high temperature kills the organism by acting on its food-medium *outside* the cell, the above is an argument against any such simple explanation of the action of the blue-violet rays, especially since experiments with other plants point to similar destructive actions of such rays in cases where no question of a bathing food-solution can be raised—unless we choose to regard the sap in the vacuole of a living cell as such a bathing medium, as indeed it is, in a sense. It would probably, however, seem a strange proposal in the present condition of plant-physiology to refer the inimical actions of light solely to reactions of the cell sap—though the possibility could perhaps not be denied.

(2.) The second external factor to be considered is light.

The experiments show beyond all cavil that light-rays of higher refrangibility bring about the death of the spores at all temperatures worth consideration;\* in this case the curve of growth does not come into account. The evidence also shows that these rays depress the otherwise normal curve; but the difficulties begin here, because we have no means of expressing the intensity of the light used in terms similar to those used in reference to temperature.

It is proved that a light of low intensity, passing through screens which transmit only blue-violet rays, kills the young filaments at low temperatures, which in the absence of these rays does not injure the filaments at all. It is also proved that these light-rays at even higher and more favourable temperatures, seriously retard the growth of more advanced filaments, so that their curve is much more depressed than the curve of similar filaments at the same temperature but protected from the blue-violet rays.

When the temperatures are very favourable to growth, however, it is often difficult to determine quantitatively the effect of the light-rays on the curve of growth, because the latter can only be observed for a period too short for the effect of the light action to be measurable; even in these cases, however, the curve is frequently seen to be commencing its depression towards the end of the observation period, and the cultures exposed to the light are seen to be deficient in crop or in spore-production subsequently.

At temperatures above the optimum it is extremely difficult to judge of the damage due to the light apart from injury due to the temperature, but the general conclusion seems likely that high temperatures act so much more rapidly than the light that most of the disastrous effects are due to the former.

\* *I.e.*, it is no use discussing temperatures known to be dangerous on their own account.

From all this it must be inferred that temperatures from the minimum to the optimum are antagonistic to the injurious light-action, and are the more effective in this respect the nearer they are to the optimum; temperatures above the optimum, on the other hand, help in the destructive light-action, or, rather, co-operate with the light in killing, or in retarding the growth.

(3.) The third external factor which helps to complicate these curves is the nature of the food-medium, and here, again, it has been extremely difficult to obtain quantitative results. The following general conclusions seem deducible from the experiments.

Under otherwise similar conditions in the dark the doubling period seems to be lengthened by the addition of gelatine, and the optimum temperature raised somewhat. How far this is due to impeded access of oxygen, owing to its slower diffusion through a more solid medium, or a similar impeding of the action of solvent enzymes, or to mere mechanical opposition to growth, or diminished access of water or food solution to the organism, could not be decided. Experiments with stiff gelatine at lower temperatures suggest the co-operation of all these obstacles; at higher temperatures it seems probable that no mechanical opposition to growth occurs, and that since the warm fluid gelatine absorbs much water, the difficulties regarding that agent disappear also, but it is not improbable that diffusion is still slower than in broth.

Whether the shorter doubling period and lower optimum in broth are due entirely to the absence of the above obstacles, or to the food-materials being really in a more suitable condition for direct assimilation by the cells, must also remain undecided. That the latter factor is not unimportant, however, is borne out by a few experiments made with less suitable food-liquids—*e.g.*, glucose solutions—and is, of course, likely on other grounds.

(4.) That access of oxygen is necessary for the growth follows from direct experiments, and also from the extraordinary depression of the curve when another oxygen-consuming bacterium gained access to the drop. Experiments where KHO was put into the arms of the cells did not support the idea that it is the mere accumulation of CO<sub>2</sub>, due to respiration which ultimately causes the depression of the curve at high temperatures, however, and further experiments are needed to show how far, if at all, the organism can endure partial pressures of oxygen.

(5.) Some of the experiments, especially at higher temperatures, indicate the importance of water, the general necessity of which will of course not be disputed.

The curves seem to be affected by sudden dilutions of the drops, owing to condensation of water on the cover-slips of the culture-cells. How far this is due to mere dilution of food-materials, enzymes, &c.,

or to osmotic phenomena, or to minute changes of temperature incident to the precipitation, cannot be determined. I have discussed the collateral effects of these condensations, as difficulties in the observations, in their place, but special experiments are needed in regard to the other points.

(6.) In a few cases there is positive evidence that volatile antiseptic substances—*e.g.*, from scorched cotton wool—inhibit the growth. Here, again, special investigations are needed to determine the effects of these on the curves, a line of experimental inquiry for which the method is admirably suited.

"An Attempt to Cultivate Parasitic Protozoa from Malignant Tumours, Vaccinia, Molluscum Contagiosum, and certain Normal Tissues, together with Infection Experiments carried out with the Culture Media, and a Note on the Treatment of Cancer." By SAMUEL G. SHATTOCK and CHARLES A. BALLANCE. Communicated by Sir JAMES PAGET, Bart., F.R.S. Received March 25,—Read May 2, 1895.

. (Abstract.)

In a previous communication the authors showed that no organism belonging to the protophyta could be cultivated from malignant new growths, and in addition that carcinomatous and sarcomatous tumours from the human subject could not be transplanted so as to produce infection in the lower animals.

About this time the researches of Nils Sjöbring and Soudakewitch, and in this country, of Ruffer and others, showed that in sections of carcinoma stained by special methods there were present certain bodies which the above observers alleged to be parasitic protozoa. The authors then determined to try whether any protozoon could be cultivated from malignant new growths; and as it is well known that the *habitat* of the common amœba is damp sand or pond water, they decided to select sand and water as the medium for their investigation. Their first experiments were imperfect, for the reason that sufficient care was not used in the sterilisation of the materials and in the precautions taken during microscopic examination.

The only experiments in which they found living amœbæ were certain of the earlier, in which a possibility of external contamination was not rigidly excluded.

The following is their final method of procedure:—

*Sand.*—Silver sand, from which the finest part had been removed by sifting, was baked in a shallow thin iron dish over a large ring Bunsen for an hour. It was then transferred to the small deep capsules and Petri dishes about to be used, which had been previously baked for an hour at 150° C. in the hot-air steriliser. The capsules so charged were then baked for an hour at 150° C.; on removal from the steriliser the sand was heaped up on one side by shaking the capsule so that when the water was added part of the sand was submerged and part remained above the level of the fluid. The object of this proceeding was to obtain a littoral in order to ensure better aëration for any protozoa that might develop.

*Water.*—This was distilled and collected in a sterilised flask; it was subsequently boiled for from four to five hours with the object of rendering it quite sterile.

*Transference of the Tumour.*—The malignant tumour was received fresh from the operating theatre, the redundant tissue around was removed with sterilised scissors; then, with knives previously sterilised at 150° C. for an hour in an iron box, pieces of the growing edge were cut away, and transferred with sterilised forceps to the capsules; they were laid on the sand just beneath the water level. Two kinds of capsules were used: one, the ordinary Petri, the other considerably deeper, of less diameter, and furnished sometimes with a cover, like the Petri, at other times not.

*Storage of the Capsules.*—The capsules thus prepared and “infected” were placed between sterilised double dishes; the covers of these dishes were raised for a short distance by means of blocks of wood which had been soaked in solution of corrosive sublimate; the height was such as to allow free entrance of air, but not sufficient to expose the mouth of the lower dish. The double dishes were first sterilised by washing with sublimate solution, absolute alcohol and by heat.

The double dishes were finally placed, each pair, upon a sheet of glass beneath a capacious shade, both of which had been cleansed with sublimate solution. Most of the small, deep capsules had their covers removed as they were placed between the double dishes. The Petri capsules remained covered throughout. All the experiments were conducted in a private laboratory continuously heated.

*Method of Microscopical Examination.*—A glass rod and slide were sterilised in the flame, and allowed to cool. The shade was removed and the upper dish raised sufficiently to allow of the passage of the rod to the capsule. A little sand was then taken from three or four places along the littoral or from the neighbourhood of the piece of tumour and transferred to the slide; occasionally a hole was dug with the rod above the water level, and some of the deeper sand removed. The sand so removed was gently stroked with the rod on the slide until displaced from one end to the other; the slide was finally inclined so that enough fluid left the sand to make a microscopic preparation.

The examination was made with 1/12 apochromatic oil immersion, Zeiss, oculars 4 and 8. Occasionally a few drops of beef peptone broth were added to the capsule; and as the water became low from evaporation more was supplied.

In all the capsules bacteria developed, a fact which the authors regard as important inasmuch as such would furnish a pabulum for the growth of any protozoa that might develop.

The authors then give a table exhibiting the results of experiments made in twenty-three capsules; there were used nine scirrhus carcinomata of the breast, and five sarcomata from different sources; the sarcomata comprised one from the human biceps, one a mela-

notic growth of the cheek, two melanotic sarcomata from horses, and a spindle-celled mammary sarcoma from a dog. In the case of carcinomata the authors confined themselves to the typical scirrhus of the breast for the reason that in new growths involving superficial parts as the lip, tongue, &c., there is not only a chance of protophytic contamination, but also of protozoic, especially as certain protozoa are normal inhabitants of such mucous passages as the vagina and intestine.

*The result of all these experiments was negative.* No traces of protozoic life, whether as spores or amœbæ were encountered, although the examinations were made at regular intervals and repeated for periods of many months.

It may be added that a similar method of investigation carried out with *normal tissues* was equally negative of result.

The experiments so made were nineteen in number: seven were with human tissues (five subjects), muscle, pancreas, spleen, mamma; and twelve with the tissues of three dogs, submaxillary salivary gland, muscle, testicle, pancreas, kidney, and spleen.

The authors obtained equally negative results with vaccinia, molluscum contagiosum, the pancreas of *Salamandra maculata*, and muscles of the frog.

*Vaccinia*.—The experiments were made with freshly excised skin of the calf on which mature vesicles had been raised. They were carried out because bodies similar to those viewed as parasitic in carcinoma have been demonstrated in the epithelium of the vaccine and variolous vesicle.

*Molluscum contagiosum* was experimented with, because certain observers have held that the "molluscos bodies" in the lesions are protozoa.

*Pancreas of Salamandra maculata* was used because it has been alleged that the paranucleus seen in certain of the epithelial cells is a protozoon.

*Muscles of the frog*; because it has been stated that active amœbæ may readily be raised by some such method as that described from the tissue in question.

The experiments of the authors, conducted with the precautions detailed in the paper, especially the avoidance of contamination from the integument, prove that this is untrue.

The general conclusion the authors draw from the different series of experiments recounted in the paper is that by the method adopted no protozoa can be cultivated from the healthy living tissues, from malignant tumours (at least such as are not directly exposed to external contamination), from the lesions of vaccinia and molluscum contagiosum, from the salamander's pancreas, and from the muscles of the frog.

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The authors record, in addition, a certain number of experiments made upon animals by means of sand infected with carcinoma and sarcoma, and containing amœbæ which later experiments showed to be adventitious. These experiments, which were all negative in result, included intravenous injection (dogs), the repeated "vaccination" of skin (white rats), intraperitoneal insertion (white rats).

Having previously found it impossible to raise a growth of carcinoma in any of the lower animals by transplantation of recent human carcinoma, they thought it possible that if the tumour was first incubated outside the body, the hypothetical protozoon might pass into some phase which would enable it to convey the infection. With this object, pieces of carcinoma incubated at the room temperature in milk, potassium oxalate plasma, and dilute broth were inserted into the peritoneal cavity of white rats, but with negative result.

In the case of two rats, the material used consisted of scirrhus carcinoma, which had been buried in a country garden for six weeks; the animals were kept alive for six months, but remained unaffected with the disease.

Under the head of Treatment are recorded the negative results following the subcutaneous injection of fresh aqueous extract of carcinoma and sarcoma in cases of both these diseases, as well as the similar injection of fresh sheep-serum.

## OBITUARY NOTICES OF FELLOWS DECEASED.

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ARTHUR CAYLEY was the second son of Henry Cayley and Maria Antonia Doughty; he was born at Richmond, in Surrey, on 16 August, 1821.

The family, to whose fame so much honour has been added by one of the greatest mathematicians of all time, is of old origin and illustrious descent. Its name, like not a few English names, is derived from a locality in Normandy; there was a *Castellum Cailliei* near Rouen held by baronial tenure. The head of the house appears to have come to England with William the Conqueror and to have settled in Norfolk, becoming Lord of Massingham, Cranwich, Brodercross, and Hiburgh in that county. The influence of the family increased and, by the time of Edward II, Sir Thomas de Cailli possessed estates also in Yorkshire. On his decease without issue, the Yorkshire property was transferred to a younger branch of the family and was inherited by a long succession of Cayleys who made their home at Thormanby. One of these was knighted, as Sir William Cayley, in 1641; in 1661 he was created a baronet in recognition of his services during the Civil Wars, the title surviving to the present day. The fourth son of Sir William, Cornelius, settled at York; and the eldest son of the latter, also Cornelius, born in 1692, was a barrister and in 1725 was appointed Recorder of Kingston-upon-Hull, an office which he held until a few years before his death in 1779. Probably the advantages offered by Hull, then, as now, the greatest port on the northern coast of England, suggested commerce as an occupation for some members of the Recorder's large family; two of his sons became Russia merchants, settling in St. Petersburg. The younger of these, being the fifth son of the Recorder, was Henry Cayley, born in 1768; he married, in 1814, Maria Antonia Doughty, a daughter of William Doughty. The eldest son of this marriage died in infancy. The youngest son, Charles Bagot, was a scholar, possessed of linguistic genius; he was particularly interested in the Romance languages and he made verse-translations of Homer's *Iliad*, Dante's *Divine Comedy*, and the *Sonnets* of Petrarch. The second son was Arthur, the subject of the present sketch; he was born during a visit of his parents to England. Before passing to the details of his life, it may be added that the second of his father's sisters married

Edward Moberly—also a Russia merchant living in St. Petersburg—and was the mother of the late Dr. George Moberly, Bishop of Salisbury.

Mr. Henry Cayley took his young family to Russia and remained there for a few years. On retiring from business in 1829, he returned to England and settled into residence at Blackheath. Arthur was sent soon afterwards to a private school there, kept by the Rev. G. B. F. Potticary; and when he was fourteen he was transferred to King's College School, London. At a very early age he had begun to shew some of those preferences by which the existence of mathematical ability is wont to reveal itself; he had a great liking for numerical calculations and he developed a great aptitude for them.

In his new school the boy showed himself to be possessed of remarkable ability: his power of grasping a new subject very rapidly and of seizing its central principles was certainly unusual. An old friend tells of an examination in chemistry: the subject had not been studied by Cayley before, but he soon acquired sufficient knowledge to carry off the medal from the professedly chemical students, to their surprise and mortification.\* But it was most of all by the indications of mathematical genius that he astonished his teachers. It had been Mr. Cayley's intention to educate his son with the view of placing him in his former business—an intention not abandoned without reluctance. The impression, however, produced upon his teachers could not lightly be set aside; and the advice of the Principal to send him to Cambridge, where his abilities promised to secure brilliant distinction, was adopted.

Accordingly, he went to Cambridge. He was entered at Trinity College on 2nd May, 1838, as a pensioner, and he began residence in the succeeding October at the unusually early age of seventeen. He passed through the ordinary stages in the career of a successful student of mathematics. Like the other able undergraduates of his period, he "coached" with William Hopkins of Peterhouse who has been described as a great and stimulating teacher—a description justified by the high achievements of a long line of distinguished and grateful pupils.

Cayley's fame grew rapidly: and, as is the way of Cambridge undergraduates, he soon was pointed out as the future Senior Wrangler of the year. It is interesting to find a record of him written about

\* It may be added that he maintained his interest in chemistry throughout his life, and acquired a considerable knowledge of it. When he was at Baltimore, in 1892, lecturing at the Johns Hopkins University by special invitation, he attended Professor Remsen's lectures with a pleasure which found expression in his letters home to his children in England. And on one occasion, at Professor Remsen's request, he lectured to the chemistry class on the hydrocarbon "trees" (*'Brit. Assoc. Report,' 1875, pp. 257—305*).

this time and published not long afterwards by an acquaintance,\* who says that :—

“As an undergraduate he had generally the reputation of a mere mathematician, which did him great injustice, for he was really a man of much varied information, and that on some subjects the very opposite of scientific—for instance, he was well up in all the current novels, an uncommon thing at Cambridge where novel-reading is not one of the popular weaknesses.”

Novel-readers are more frequent in Cambridge now than they appear to have been in 1842, and Cayley in his later days avoided reading some of the modern novels; but it is worth noting, as will subsequently be seen more in detail, that he had this “popular weakness” all his life.

He was admitted a scholar of the College on 1st May, 1840, winning his scholarship at the earliest time when it was possible to do so: and he secured a first class in each of the annual examinations of the College. No record of marks for the first and the second years is given in the Trinity Head Examiner's Book; but in the third year the marks are given and, as he then scored more than twice the marks of the second candidate, the Head Examiner separated him from the rest of the first class by drawing a line under his name. This presage of his powers was confirmed in the following year, 1842, when he graduated as Senior Wrangler; the Examiners were so definitely satisfied that he was first as to dispense in his case with the viva voce tests which at that time were a customary part of the Tripos. And in due course the first Smith's Prize was awarded to him in the succeeding examination.

Cayley's own “year” at Trinity was a distinguished one; for, in addition to himself, it contained Mr. (now the Right Honourable) George Denman, for many years a Judge of the High Court of Justice, and Hugh Andrew Johnstone Munro, one of the foremost of Latin Scholars of any period. And the distinction of Cayley's contemporaries in neighbouring years is marked: it is impossible to avoid noticing the names of some of the graduates in the Mathematical Tripos about that time. Sylvester and Green (second and fourth wranglers respectively in 1837), Leslie Ellis (senior in 1840), Stokes (senior in 1841), Cayley (senior in 1842), Adams (senior in 1843), Thomson—now Lord Kelvin—(second in 1845), constitute an extra-

\* Bristed, ‘Five Years in an English University’ (second edition, 1852), p. 95.

It may be added that Cayley declared the story about him in the tripos, recorded by Bristed, to be quite apocryphal.

So also was another story, belonging to a later part of his life, according to which he is reported to have said that “the object of law was to say a thing in the greatest number of words, and of mathematics to say it in the fewest”: this view, and the possibility of his ever having held it, he repudiated entirely.

ordinary succession of mathematicians of whom England is justly proud. Their achievements in mathematical science have done much to render their University one of the acknowledged chief mathematical schools of the world.

Cayley was elected a Fellow of Trinity and admitted to fellowship on 3rd October, 1842, at an age younger than any other fellow of the college, at least in the present century; and he was promoted from the position of Minor Fellow to that of Major Fellow on 2nd July, 1845, the year in which he proceeded to his M.A. degree. He was an Assistant Tutor of the College for three years; but such a post was then of an almost nominal character, and there appears to be no indication that any of the mathematical teaching of the College fell to him. He did, indeed, accept some private pupils: his life-long friend, Canon Venables, has given a pleasant account\* of a reading-party which Cayley took to Aberfeldie in 1842.

His pupils, however, did not tie him strictly to Cambridge, for it appears that the latter half of the year 1843 was devoted to continental rambles. The summer was spent in Switzerland where his zest for walking and for mountain-climbing, a pleasure that never failed while his health lasted, found an active outlet: he had become a member of the Alpine Club in its comparatively early days. The last four months of the year were spent in Italy, partly in the North and in Florence, partly in Rome and Naples. It may have been on this tour that he acquired his love for both painting and architecture. The works of painters such as Masaccio, Giovanni Bellini, Perugino, and Luini, then first became known to him; they proved a delight at the time and remained a happy remembrance with him.

These and other continental journeys from time to time, while he remained in residence as a Fellow of his College, were his relaxations. He had no formal lecturing and he did not attempt to obtain a large number of private pupils. The leisure that he thus secured was turned to the best, and to him the most pleasant, of uses, in carrying out mathematical researches. It was, indeed, as an undergraduate that Cayley began the marvellous series of publications which, extending over more than fifty years of his life, have been concerned with practically every branch of pure mathematics as well as with theoretical dynamics and physical astronomy.

The time seemed ripe for the outburst of some mathematical activity. By the efforts of Herschel, Peacock, and Whewell, Cambridge teaching had been set free from the bonds that restricted methods of procedure to those which had proved effective in Newton's days; and the struggle to secure the admittance of analytical methods had been successfully completed. One sign of the new freedom was the foundation of the 'Cambridge Mathematical

\* 'Guardian,' 6th Feb., 1895, p. 201.

Journal,' in 1837, by D. F. Gregory and Leslie Ellis. Before that time, practically the only English means of publication open to mathematicians was in the 'Philosophical Transactions of the Royal Society'; and young writers, whether modest or not about the value of their researches, might well have hesitated before seeking publication in a quarter that exacts so high a standard. The new journal then founded was open to young students and gave them an opportunity, previously difficult to obtain, of making their researches known; and it proved a great stimulus to the intellectual activity of those members of the University. Only four volumes of the journal appeared; but it was continued, first under the name of the 'Cambridge and Dublin Mathematical Journal,' and, subsequently down to the present time, under that of the 'Quarterly Journal of Pure and Applied Mathematics.' Though the opportunities of publication, which now are afforded to mathematicians both in England and abroad, are vastly more numerous than they were half a century ago, the undoubted service rendered to English mathematics by the initial venture of the two young Cambridge men should not be forgotten.

It was in the second volume of this journal that Cayley's earliest paper, written in 1841, was printed: and two other papers bearing the same date—it was the year before his degree—are included in the third volume. Though the results are not remarkable, the freshness and the independence of these early investigations are worthy of notice. Cayley had evidently read with enquiring and critical care the "*Mécanique Analytique*" of Lagrange, some of the work of Laplace, and several memoirs in the two continental journals of the time, those of Liouville and Crelle. These achievements of an undergraduate of nineteen or twenty, which are rarely accomplished now and were still rarer in his day, recall Abel's dictum\* :—

"Si l'on veut faire des progrès dans les mathématiques, il faut étudier les maîtres et non pas les écoliers."

It was as certainly one of the characteristics of Cayley to find a stimulus to new developments in the main ideas of other writers as it was one of his characteristics to be able to follow out his own ideas with the insistent unwearying patience of an investigator creating a new work complete. And it is interesting to see how this faculty of receiving inspiration reveals itself from the beginning of his career.

Once free from the necessity of preparing for his Tripos and his Fellowship examination, he was able to throw himself into the work of production. His activity may be estimated from the fact that he produced three papers in 1842, eight in 1843, four in 1844, and thirteen in 1845. Moreover, these papers deal with a great variety of subjects. Thus he makes his first investigations in the numerative

\* 'Niels-Henrik Abel' (par Bjerknes, Paris, 1835), p. 173.

calculus of plane curves: he initiates his discussions about geometry of  $n$  dimensions: he founds the theory of invariants and covariants: and he elucidates the connexion between doubly-infinite products and elliptic functions. Some of these early papers are now classical; and the briefest inspection of them is sufficient to reveal the suggestiveness and the easy strength of the young mathematician who was not yet in his twenty-fifth year.

Even by this date the opportunities of publication in England had become inadequate to his needs. Curiously enough, he does not appear to have sent any paper to the Royal Society until the year 1852, when Sylvester communicated the "Analytical Researches connected with Steiner's Extension of Malfatti's Problem,"\* to the Society. Later in the same year, Cayley was elected a Fellow of the Society, and thereafter many of his papers appear in its 'Transactions.' Before 1852, there were few journals either at home or abroad which did not receive communications from him: and even in the quite early years of his researches, several of his papers, written in French, appeared in Liouville's journal and in Crelle's journal. As societies and journals grew in number, so the area over which his papers spread became ever wider.

At first, after winning his Trinity Fellowship, he remained at Cambridge, and his time must then have been largely at his own disposal. This freedom, in his circumstances, could last only for a limited time, because, unless he either entered holy orders or devoted himself to teaching in some permanent post (if obtainable) in the College, the Fellowship could be held for not more than seven years after his M.A. degree—a period that would expire in 1852. He was unwilling to take holy orders—not that there was any religious obstacle in his way, for he was not harassed either by philosophical doubts or critical difficulties. His simple reason for remaining a layman was that, though devout in spirit and an active Churchman, he felt no vocation for the sacred office.

In consequence, it became necessary to choose some profession. Cayley selected the law, left Cambridge in 1846, entered at Lincoln's Inn, and became a pupil of the famous conveyancer, Mr. Christie. A story of their first interview, that Mr. Christie used to tell in after years, is an illustration of the modesty and the lack of self-assertiveness which were leading features of Cayley's character: and this impression is confirmed by the recollections of a fellow-pupil, Mr. T. C. Wright, who says:—

" . . . We fellow-pupils knew that Arthur Cayley had been the Senior Wrangler of his year, and that he possessed extra-

\* Cayley's 'Collected Mathematical Papers,' vol. 2, No. 114. Subsequent references to this series will be made in the form 'C. M. P.'

ordinary abilities; but they were not indicated by his personal bearing, and the retiring modesty of his disposition prevented him from ever alluding to the honours he had won at Cambridge. He had one of the most unsophisticated minds I have ever known; jokes, and the badinage of the pupil-room, seemed to be delightful novelties to him, and his face beamed with amusement as he listened to them without taking much part in the conversation, being content to devote his time assiduously to work which I suspect was not altogether congenial to his taste. . . ."

But if the modest, almost shy, man did not display his honours, he could not conceal his powers; and very soon his clearness of head, his almost intuitive grasp of the principles of any subject that came before him, his capacity for work and his power of concentration, made him a favourite pupil. He was called to the Bar on 3rd May, 1849, and thereafter he had no occasion to wait for business. Mr. Christie was always ready to supply him with at least as much conveyancing work as he was willing to undertake: but no advice, no encouragement, no opening however favourable, least of all any wish for fame or fortune, could tempt him to subside into a large practice. He restricted himself to "devilling" for Mr. Christie, and he limited the amount of work he would undertake in this way, always refusing work that came to him at first hand. There is no doubt that, had he remained at the Bar and devoted himself to its business, he could have made a great legal reputation and a substantial fortune: even as it was, some of his drafts\* have been made to serve as models. But the spirit of research possessed him; it was not merely will but an irresistible impulse that made the pursuit of mathematics, not the practice of law, his chief desire. To achieve this desire, he reserved with jealous care a due portion of his time; and he regarded his legal occupations mainly as the means of providing a livelihood.

He remained at the Bar for fourteen years. Between two and three hundred papers are the mathematical outcome of that period; and they include some of the most brilliant of his discoveries. Among these papers are to be found the majority of his famous memoirs on *quantics* (particularly the sixth memoir, in which he develops his theory of geometry and shows that all geometry can be made entirely descriptive), his work upon *matrices*, numerous contributions to the theory of symmetric functions of the roots of an equation, the elaborate calculations connected with the development of functions arising in the planetary and the lunar theories, and his valuable reports on theoretical dynamics. The enormous range over which

\* In Davidson's 'Precedents and Forms in Conveyancing' (third edition, 1873), vol. 3, Part II, p. 1067, the author adds a footnote, calling "attention to the remarkable skill exhibited in [a] settlement, the work of Mr. Arthur Cayley."

his papers of these fourteen years extend is not more remarkable than the vigour of his contributions to knowledge; and a reference to them will show that he frequently recurs to some given problem, always adding something to the development.

In judging of this persistent and unflagging activity, some account ought to be taken of his surroundings. It can hardly be that 2, Stone Court, from which many of his papers are dated, proved an inspiration to mathematical research. For part of the time, his friend Sylvester was in London—then as an actuary; and I have heard Cayley describe how Sylvester and he walked round the Courts of Lincoln's Inn discussing the theory of invariants and covariants which occupied (and occasionally absorbed) the attention of both of them during the fifties. And on matters which related to analytical geometry he was in frequent (but formal) correspondence with Salmon; indeed, the relation that existed between the two men developed ultimately into one of warm friendship and deep mutual regard: its sincerity can be gathered from the spirit animating Salmon's notice of Cayley, published in 'Nature' in 1883, at the time when the latter was President of the British Association. But, with special exceptions of the types indicated, his work was so largely of the kind that is called path-breaking that he was bound to do it alone: he did it with a simple unconscious courage and with unfailing resolution.

It may easily be imagined that his links with life at Cambridge had now become slight. During the earliest of the years spent at the bar, he had returned on a few occasions. In 1848, the year before his call, he was the junior mathematical examiner in the regular annual examinations of Trinity; in 1849, and also in 1850, he was the senior mathematical examiner in the same examinations. In 1851 he was Senior Moderator for the Mathematical Tripos; one of the wranglers, Lightfoot, becoming subsequently his friend, and his colleague in the University, before going to his great work in the diocese of Durham as Bishop. In 1852 he was Senior Examiner for the Tripos, the senior wrangler of the year being Tait (also afterwards one of his intimate friends), now Professor of Natural Philosophy at Edinburgh. These seem to have been the only occasions when he was recalled to Cambridge; and they did not require any permanent connection with the College or the University. He was settled in London, his allegiance divided between law and mathematics.

A change, however, in the statutes of the University offered an opportunity for his return to Cambridge; a professorship of pure mathematics was established upon an old foundation. Lady Mary Sadleir (who endowed the Croonian Lecture Fund of the Royal College of Physicians of London, and also that of the Royal Society, in memory of her first husband, Dr. William Croone, a physician and

one of the earliest Fellows of the Royal Society) had, by her will, dated 25th September, 1701, and proved 6th November, 1706, given to the University an estate, which was to be used as an endowment of lectureships in algebra at nine of the colleges in Cambridge. These posts were duly established. The great developments of analysis, which took place at the end of the last century and during the first half of the present century, gradually proved that the restriction to algebra prevented the lectureships from being as adequate an encouragement to the advancement of mathematics as they were designed to be at the time of their establishment. Moreover, the lecturers had ceased to attract undergraduates to their lectures: so that the purpose of the foundation was not being fulfilled. Consequently, in 1857, a proposal was made by the Council of the Senate of the University that a new direction should be given to the endowment by the establishment of a professorship, to be called the Sadlerian Professorship of Pure Mathematics: the duty of the professor was "to explain and teach the principles of pure mathematics, and to apply himself to the advancement of that science." The proposal was approved by the Senate on 3rd December, 1857, and the new statute was sanctioned by an Order of the Queen in Council on 7th March, 1860. Some time had to elapse before certain provisional arrangements could be completed, and it was not until after three years that the University was in a position to act.

On 10th June, 1863, Cayley was elected Sadlerian professor: he held the chair for the rest of his life. The stipend attached to the professorship was modest, though it was improved in the course of subsequent legislation; these changes, however, could not have been foreseen at the time when Cayley was elected. Yet he had no hesitation about returning to Cambridge: for the post enabled him to devote his life to the pursuit he liked best. He never felt the slightest regret at having neglected the prospects of distinction at the bar, or at having chosen to return to his University; and he always expressed perfect satisfaction and content with his life in Cambridge, which was one of great happiness.

His appointment as Sadlerian professor marks a turning point in his life. Henceforward he lived, for the most part, in the quiet of the University, though by no means in seclusion, for he took his share in administration, which claims a part (often too large a part) of the leisure of men fitted for this necessary duty. But he was not burdened by heavy claims arising out of his official position: and he was directed by the statutes governing him to do what was, as a matter of fact, his ideal in life. No man could have been better suited than Cayley was to fulfil the charge of the statutes: his knowledge and his power of research pointed him out as the obvious choice of the electors.

He settled in Cambridge at once. On 8th September, 1863, he married Susan, daughter of Robert Moline, of Greenwich. This is not the place to dwell upon his domestic life; but it is impossible to omit in silence all reference to its singular happiness, based upon the affection felt by its members for one another. Friends and visitors who have been in that home will not soon forget the kindness and the gracious courtesy of the welcome they received, or the atmosphere of peace into which they were raised. Sometimes in the old garden by the riverside, more often in the drawing-room, the talk went on; the professor himself listening, attentive and watchful, frequently taking only a slight share, but ever ready to join in. No cynicism or paradox in speech was ventured upon in his presence; no harshness of judgment was tolerated without a quiet protest; no sense of bustle or ambition was felt there; in all things the charm of an old-world home, centred round him. His widow and their two children, Mary and Henry, remain to mourn their loss.

His teaching duty was limited to the delivery of one course of lectures in the academic year, and he usually chose the Michaelmas term. This practice was maintained for twenty-three years until he was placed under the new statutes, which in 1882 had come into operation, so far as concerned all future appointments. After that change, he delivered two courses of lectures, one in the Michaelmas term, the other in the Lent term. An inspection of the list of his lectures shows that he chose his subjects by preference from analytical geometry, dynamics (in his view, theoretical dynamics is a portion of pure mathematics), differential equations, theory of equations, Abelian functions, elliptic functions, and modern algebra. The titles of the lectures, as announced, were sometimes vague, nor were they intended to limit his range; in all cases he went far beyond the boundary that so frequently limits Cambridge studies. Thus a course of lectures on differential equations, announced for the Michaelmas term in 1879, was chiefly concerned with conformal representation, polyhedral functions, and Schwarz's investigations on the hypergeometric series.

For many years he dispensed with the use of blackboard and chalk in his class-room; this was possible because his class usually was small. He brought his work written out upon the blue draft-paper\* which was regularly used by him in all his writing of mathematics; the exposition consisted partly of verbal explanations made as he showed the manuscript, partly of details written out at the moment. A change came in 1881, when his class amounted to fifteen or sixteen: he was then obliged to use the blackboard, and he subsequently maintained the new practice. Occasionally his older habit of explaining his manuscript recurred—he then placed it upon the board. This

\* It was the customary "scribbling paper" of his undergraduate days.

was especially the case when he brought carefully prepared diagrams, such as those used in the modular-function division of the plane: these diagrams were made much clearer by the use of water-colours to distinguish different sets of regions, and their preparation evidently gave him pleasure.

But as may be surmised his influence as a teacher was overshadowed by his influence as an investigator. Those whom he affected by his lectures belonged for the most part to the mathematical teachers in Cambridge: the number of undergraduates whom he influenced was small, though, when any one of them did come under his influence, the effect was well marked. His starting point in any subject was usually beyond the range of all other than quite advanced students; but to any able undergraduate who was willing to devote time, not merely to the comprehension of the matter in the lectures but also to collateral reading, the lectures were stimulating and inspiring. This effect was partly due to the easy strength with which he worked, partly to the spirit in which he approached old and new subjects alike; an independent suggestiveness and a singular freshness marked his views, and gave an added interest to his exposition even of a well-known theory. One reason of this freshness may be found in the fact that his lectures consisted of the current researches upon which he was engaged at the time; sometimes, even, a lecture would be devoted to results which he had obtained since the preceding lecture. Though the titles of the courses occasionally recur from one year to another, the same course was never given twice. The new matter in any course, once given, was usually incorporated in a paper or memoir; and when the same subject was nominally lectured upon again, it was a distinct part of the subject—old notes were never used a second time.

It was not alone by his lectures that he acted as professor. Students, seeking help or desiring to interest him in their work, found him always willing to give them the benefit of his advice, his criticism, and his knowledge. Nor was it merely mathematicians in Cambridge whom he helped in this way. He was continually consulted by foreigners, who appreciated the promptness no less than the fulness of information in his replies.

It frequently happens that a man of genius, great enough to leave a distinct impress of his originality upon his science, finds it irksome to study what others have written. With the growth of all sciences during the last fifty years, especially—it may be said—with the growth of pure mathematics in that time, the tendency of workers is to become specialists in their own subject and, perhaps, in subjects immediately cognate with it, and to acquire only a slight acquaintance with what is being done outside the circle of their limited interests. Not so was Cayley: he was singularly learned in the work

of other men, and catholic in his range of knowledge. Yet he did not read a memoir completely through: his custom was to read only so much as would enable him to grasp the meaning of the symbols and understand its scope. The main result would then become to him a subject of investigation: he would establish it (or test it) by algebraical analysis and, not infrequently, develop it so as to obtain other results. This faculty of grasping and testing rapidly the work of others, together with his great knowledge, made him an invaluable referee; his services in this capacity were used through a long series of years by a number of societies to which he almost was in the position of standing mathematical adviser.

Concurrently with his teaching, he continued his investigations. He wrote only one book—a 'Treatise on Elliptic Functions,' published in 1876, which was intended to bridge over the gap from Legendre's 'Traité des Fonctions Elliptiques' to Jacobi's 'Fundamenta Nova'; it contains a considerable amount of new matter. But paper after paper was published in a long unfailling succession almost until his death; their tale amounts to more than 800. Happily for the convenience of mathematicians, the republication of his papers in collected form was undertaken by the Cambridge University Press—perhaps the most enduring, certainly not the least fitting, monument of his fame. The request was made to him in 1889 by the Syndics of the Press; he willingly acceded to it and deeply appreciated, both then and afterwards, what he regarded as a great compliment to himself. Seven large quarto volumes, under his own editorship, have already appeared. The preparation of them was always a great happiness to him; and, especially in the later years of his life, it gave him an occupation in his science which was still within the range of his failing strength. At the time when the collection was begun it was estimated that ten volumes would suffice for the purpose, but it is now evident that ten will be certainly insufficient. The Syndics of the Press intend to complete the series of volumes; it is a matter of regret that the illustrious author of the papers has not lived to complete it himself.

Even his teaching and investigations did not fully occupy his time. For the first few years after his return he was left comparatively free from a large share in administration, but gradually it was assigned to him. As he became better known for his effective business capacity, his share in administration grew until he came to be regarded as an indispensable member of the Council of the Senate. He was elected a member of that body on 7th November, 1876, and with the exception of some six months when he was absent in America, he continued a member of it until 1892, when failing health compelled him to resign. During this period of service he was re-elected three times; while party feeling ran rather strongly

at times during the discussions that led to the new statutes, both parties included his name among their lists of nominations—an adequate proof that he possessed the confidence of the Senate. He was free from party bias, and he became established in his position of strength by his fairmindedness, his sound judgment, and his calm temperament. He would listen to a discussion, speaking only when he had something of importance to add; when speaking he was listened to with full attention. More frequently he would take no part in the discussion until his opinion was asked, as was usually the case in difficult questions; his opinion was always valued and sometimes final. Similarly, on syndicates, his co-operation was much sought, and in particular the services which he rendered to the Library Syndicate and the Press Syndicate were of substantial importance. He also took great interest in the movement for the higher education of women. In the early days of Girtón College he gave direct help in teaching, and for some years he was Chairman of the Council of Newnham College, in the progress of which he took the keenest interest even to the last.

But, with all his general aptitude for business, he was perhaps most specially helpful by his legal knowledge. The training he had undergone and the knowledge he had acquired at the bar ultimately proved invaluable. His opinion on legal matters was sought by the University, by his own college, and by the scientific societies with which he was connected; when given, it frequently had the effect of a judicial decision. His powers of drafting were constantly being called into requisition; he responded to the calls upon him and, with unstinted generosity, placed his time and skill at the disposal of these bodies, so that the new statutes of Trinity College, and not a few of the statutes and ordinances of the University, owe much to him.

One other illustration, at once of his general business capacity and of the confidence reposed in him, may be given. The elections for representatives of the Universities in the House of Commons are still conducted openly and by means of voting papers, delivered either by the elector himself or by another elector whom he has nominated; objections may be raised against any voting paper, but they must be decided at once. In Cambridge the Vice-Chancellor, being the returning officer, nominates a number of assessors to act with him in the case of a contested election. At a bye-election in 1882, when the candidates were Mr. H. C. Raikes and Professor James Stuart, Cayley was nominated as presiding officer at one of the polling places. His imperturbable firmness, his calm courtesy, and the justice of his decisions secured for his effectiveness in this capacity the admiration of the University.

This brief account of his participation in business affairs is necessary; without some such indication a proper estimate of his position

in Cambridge cannot be framed. And it also may help to show that his supremacy in the subjects of his investigations neither made him a recluse, nor limited his other interests, nor restricted his practical usefulness.

The merits of such a man were recognised by the only means at the disposal of a grateful and appreciative University. He was elected an honorary Fellow of Trinity College on 22nd May, 1872, at the same time as Dr. Lightfoot, Mr. James Spedding, and Professor Clerk Maxwell; and on 11th October, 1875, he was made an ordinary Fellow, a position which he retained for the rest of his life. His friends subscribed for a presentation portrait,\* painted by Lowes Dickenson in 1874; it now hangs in the College Hall. The simplest of inscriptions is on its frame, but the humorous lines which Clerk Maxwell† wrote at the time should not readily be forgotten. The graver element, seldom absent from his verses, is not entirely repressed even by his wit, and the lines were based upon a deep admiration of the man

" Whose soul, too large for vulgar space,  
In *n* dimensions flourished unrestricted."

His bust, by Mr. Henry Wiles, was given to Trinity College by a donor who wished to remain anonymous. It was placed in the beautiful library of the College on 3rd December, 1888, an honour that has been conferred during life in only two other cases—Tennyson and Sedgwick.

After the new statutes came into operation, the Senate on 27th May, 1886, decided that the Sadlerian Professorship should at once be made subject to the improved provisions, a decision which, though it increased the amount of lecturing required, gave him the benefit of the full stipend. At the same time the Lucasian Professorship, held by Professor Stokes, was also made subject to the new statutes; and it was currently believed that the Lowndean Professorship would have been included in the proposal had Professor Adams been willing to have the change made. There was a wish on the part of members of the University to give some recognition to the glory conferred upon the mathematical school by Stokes, Adams, and Cayley; one possibility remained. The opportunity came in 1888 when Prince Edward (as he was known in Cambridge), afterwards Duke of Clarence, received the degree of LL.D. Such an occasion is customarily marked by the conferment of a number of honorary degrees upon distinguished men; among them, on this particular occasion, were the three professors who had been colleagues for a

\* A photographic reproduction of the portrait is prefixed to vol. 6 of the 'C. M. P.'

† See Campbell and Garnett's 'Life of James Clerk Maxwell,' p. 636.

quarter of a century. On the 9th of June in that year a great assembly gathered to see these degrees conferred upon the recipients. It need hardly be said that the men singled out for honour received ovations on being presented; among the most enthusiastic ovations were those accorded to the three professors.

Nor were external bodies and learned societies, both at home and abroad, backward in recognising the merits of his work; the honours he received were numerous and came from all quarters. Honorary degrees were conferred upon him by several universities as well as his own, among them being Oxford, Dublin, Edinburgh, Göttingen, Heidelberg, Leyden, and Bologna. President Carnot nominated him an Officer of the Legion of Honour. He was either a Fellow or a foreign corresponding member of most of the scientific societies of the Continent, among them being the French Institute, the Academies of Berlin, Göttingen, St. Petersburg, Milan, Rome, Leyden, Upsala, and Hungary. He was also a Fellow of the Royal Society of Edinburgh, of the Royal Irish Academy, and of the Royal Astronomical Society. He had been President of the Cambridge Philosophical Society, and he sat on its Council for many years; also President of the London Mathematical Society, and of the Royal Astronomical Society. He was elected a Fellow of the Royal Society on 3rd June, 1852, and he served as a member of its Council for six periods of office. He received from the Royal Society a Royal medal in 1859 and in 1882 the Copley Medal, the highest scientific distinction it is in its power to bestow. When the De Morgan Medal was instituted in connection with the London Mathematical Society, the first award was fitly made to Cayley. And from Leyden he received the Huyghens Medal.

Mention should be made of one other honour which he received: it is of a kind seldom conferred. The high opinion of his work which was held in America was indicated by an invitation in 1881 to deliver a course of lectures in the Johns Hopkins University, Baltimore, where his friend and fellow investigator, Sylvester, was then professor. He accepted the invitation, and left England in December of that year. During the next five months he lectured on Abelian and Theta Functions; the substance of these lectures was incorporated in a memoir subsequently published in the '*American Journal of Mathematics*.\*' He returned to England in June, 1882, bringing back pleasant remembrances of kindnesses and friendships.

His life, spent in mathematical research and in the quiet round of activity in the University, offered little of either interest or incident to make his name known by the outside world to the same extent or

\* Vol. 5 (1883), pp. 137—179; vol. 7 (1885), pp. 101—167.

in the same way as the names of many scientific men, engaged in other lines of inquiry, are known. Once, however, in his life circumstances brought him prominently into notice. In 1883, he was President of the British Association for the Advancement of Science, the meeting being held at Southport; and, in that capacity at the opening of the meeting, he had to deliver a formal address, an abstract of which appeared as usual in the leading newspapers of the country.

In the early days of the Association, the President's address frequently reviewed the whole field of science; but as knowledge has developed, a tendency has set in, according to which each later President has confined himself more particularly to those matters within whose range he is an authority. And, subject to this restriction, it is hoped that the address may be legitimately popular. There have been critics of presidential addresses prepared to assert that science was sacrificed to popularity; there have been immense audiences convinced that popularity was sacrificed to science. Taken together, the presidential addresses, some severe and others popular, form an interesting series of reviews of the successive stages in scientific achievements.

Cayley's address belonged to the severely scientific class. From the nature of his subject—the progress of mathematics, more particularly of pure mathematics—it was bound to have this character. Few of the members of a regular Association audience have more than a slight acquaintance with pure mathematics; and, consequently, it is impossible to deliver to such a gathering an address which, in a reasonable time, can give them any real idea of the condition or the progress of the science. Cayley felt this and confessed to the feeling in a passage which is perhaps the best known in the address:—

“It is difficult to give an idea of the vast extent of modern mathematics. The word ‘extent’ is not the right one: I mean extent crowded with beautiful detail—not an extent of mere uniformity such as an objectless plain, but of a tract of beautiful country seen at first in the distance, but which will bear to be rambled through and studied in every detail of hillside and valley, stream, rock, wood, and flower. But, as for everything else, so for a mathematical theory—beauty can be perceived but not explained.”

But he also felt that the respect due to the Association requires its President to deal with that branch of science about which, as he knows it best, he is best fitted to tell them, so that different subjects may thus in turn be brought before successive meetings.

"So much the worse," he added, "it may be, for a particular meeting; but the meeting is the individual which on evolution principles must be sacrificed for the development of the race."

Granting then the inevitably stern character (as popularly estimated) that must mark any proper exposition of his subject, the address is one of singular interest. It undoubtedly made a great impression. Parts of it were incomprehensible to all but mathematicians: still, there was much which others could understand and, understanding, found excellent. Even leader-writers at the time recognised its lucidity, its finish, its native elegance, and its instructive and stimulating essence. To mathematicians it counts for much. Not merely is it a valuable historical review of various mathematical theories; but the exposition possesses all the freshness, the independence of view, the suggestiveness and the amazing knowledge that were so characteristic of Cayley. And, consequently, it can often be recurred to with unfailing profit.

After this event, his life pursued the unbroken tenor of its scientific course. Ever thinking, working, writing, he maintained the flow of his papers with the same unslackening vigour, and he showed the same sympathetic encouragement of others, as had marked him before the scientific world had tried to acknowledge his genius by showering its honours upon him.

It is now some years since the painful internal malady, which ultimately was the cause of death, began to show itself. At first, its action was slow; and there was reasonable hope that his naturally strong constitution would enable him to throw it off. Unfortunately these hopes were not realised; its growth was steady, its undermining influence persistent. Change of scene was tried once or twice, but without good effect; and it soon appeared that Cambridge itself troubled him least. Three years ago his friends saw that his health began to fail: he had occasional attacks of severe illness which confined him to his bed for weeks together, each of them leaving him gravely frailer than before. Gradually he became confined to his house and his garden; he could see only very few friends, and usually even them only for a short time. When they did see him, they found only too clearly how rare and brief were his intervals of relief from pain, though occasionally his gentleness and his patience would almost delude them into hope.

The last of the severe attacks began on the 8th of January; he seemed to be getting better when, on the 21st, his strength suddenly began to collapse. He died about six o'clock on the evening of Saturday, 26th January, 1895. The funeral took place on the succeeding Friday when, in Trinity Chapel, a great assemblage, com-

posed of members of the University, of representatives of the embassies of Russia and America, as well as of various learned societies and of personal friends, gathered to pay him their last homage of respect and reverence.

Sufficient has been said to show that Cayley was a man of general activities; but his scientific work and his public duties by no means exhausted or limited his general interests.

It has already been stated that, as an undergraduate, he was fond of reading novels; this practice remained with him all his days. He preferred a novel of the old orthodox type with a "happy ending"; and though his greatest delight was in the older novels, a modern book, such as 'Beside the Bonnie Briar Bush' (which he read quite late in 1894), met with words of warm praise. He had a good memory, and used to discuss plots and characters with considerable animation. The two novelists, by whose works many English people are divided into one or other of two classes, did not affect him much; Thackeray he read but did not like, and he would not read Dickens. His favourite authors were Scott and Jane Austen; all their works had been read by him many times, and they were read aloud to him during the long period of his illness. 'Guy Mannering' and 'The Heart of Midlothian,' among Scott's, and 'Persuasion,' among Jane Austen's, were the books he liked the best. He also was fond of George Eliot's novels, particularly of 'Romola.' Indeed, though he had aversions, his taste was somewhat general. Commendation of a book was enough to make him willing to try it; and there was only one limitation to his range of novel-reading—he had an instinctive abhorrence of anything that suggested either coarseness or vulgarity.

His English reading was not confined to novels. He had a keen liking for many of Shakespeare's plays, notably 'Much Ado about Nothing,' and some of the historical dramas. He delighted in Milton's shorter poems, though he would not tolerate 'Paradise Lost.' Scott's poems were frequently read; and he had a great appreciation of Byron's 'Tales' and of Coleridge's 'Ancient Mariner.' Grote's 'History of Greece' and Macaulay's 'History of England' he read repeatedly and with zest; and he never seemed to tire of Lockhart's 'Life of Scott.'

He was also a good linguist. He knew French well; it was a second writing-language to him, as will be seen from the large number of papers, written in French, which occur in his collected mathematical papers. He read (but he did not talk) German and Italian with ease, and his Greek remained fresh throughout his life. This last power may have been due to the admiration he felt for Plato; he referred to the 'Republic' and the 'Theætetus' in his Presidential Address; and, on the afternoon of the day of the "Greek

division"\* in the Senate House, I remember finding him at home reading the 'Gorgias.'

He had the keenest interest, amounting almost to a passionate delight, in travelling; cities of historic or artistic fame delighted him equally with beautiful scenery. Long after he had become an invalid, he found a fascination in guide-books and maps; and all his younger friends will recall the sympathetic zeal with which he entered into their projected journeys, and the happy pleasure he took in hearing them speak of recent journeyings and in recalling, with a wonderful vivid memory, his own experiences and ideas about places they had visited.

Reference has been made to his early pleasure in the old Italian masters. Yet, if any inferences can be drawn from the likings of his later years, architecture attracted him even as much as pictures. He had a true feeling and a clear judgment as to genuine excellence: he sketched well, and had a quick eye for proportions, perspective, light and shade. One of his relaxations was to make coloured sketches of buildings that he liked, notably sepia drawings of some of the great Gothic cathedrals and churches of northern France. He kept up his practice of water-colour painting all his life, and in his closing years it proved a great solace to him at times when his strength was so far reduced that he could not work. He had great happiness in looking at architectural pictures and at books on architecture, one of his favourites among the latter being Street's 'Brick and Marble in the Middle Ages.'

Financial matters and accounts also interested him; and only a few months before his death he published a brief pamphlet on bookkeeping by double entry, which he has been known to declare one of the two perfect sciences. He could not resist some reference to the subject in his Presidential Address, making the remark that the notion of a negative magnitude "is used in a very refined manner in book-keeping by double-entry."

His bearing was gentle, and it was marked by a courtesy that was unfailing. On questions of administration and in discussions, his opinions were stated clearly and quietly. Not that he did not hold decided views or that he would abate one jot of his firm, even chival-

\* In 1891 a proposal was made by the Council of the Senate for the appointment of a syndicate to inquire, among other things, into the expediency of allowing alternatives for one of the two classical languages in the Previous Examination. Many members of the Senate were convinced that the adoption of an alternative would lead to the extinction of the study of Greek except in the greater public schools; they consequently opposed the proposal, which, on 29th October, 1891 was rejected by a great majority (525 to 185).

It may be added that Cayley was in the minority. He allowed his signature to be added to a letter which was sent to the London newspapers as an appeal for assistance in defeating the attempt to resist inquiry.

rous, defence of what he held to be right; but there was a judicial temper in his mind which prevented the subjective element in a discussion from disturbing his equanimity. The even balance of his mind enabled him to recognise and appreciate the position of one who differed from him, and his quiet "I do not think so" was all the more effective because its very calmness excluded the slightest suggestion of hostile spirit.

His figure was spare: until his illness, he could easily endure the fatigue of long walks, in which he delighted, especially in hill country. In later years it became rather bent, and he had the appearance of being frail. His head was very impressive, as may be seen from his portrait and from photographs. In repose, and when his attention was not concentrated upon what was passing, his face had a grave air and the blue-grey eyes suggested that he was far away in thought; but when attentive or amused, and when expressing pleasure, the eyes became singularly keen and a peculiar charm lightened up the whole face.

He was absolutely modest. The honours conferred on him in full profusion never injured in the least degree the grand simplicity of his character, never gave rise to the slightest trace of vanity, which was alien to his nature. He rarely spoke of them, and, when he did, it never was as of honours: they pleased him, but, perhaps, rather as recognition of his work than as tributes to the worker. If any one expressed appreciation of any of his papers, owing to the help it had given, he would reply very quietly: but he did not stint the expression of his pleasure at advances beyond his own results when they were made by others. Public appearances were rather distressing to him at first, for his disposition was retiring and he could be reserved; but as time wore on, duty often compelled him to take part in them. In such cases he accepted the claim and discharged it with a straightforward simplicity that was entirely devoid of self-consciousness; but he gladly avoided demonstrations whenever it was possible.

In the spirit of his work one great quality was his generosity to others, particularly to young men, whose work he was always willing to recognise. He ignored the fact that he was a great mathematician—probably it never occurred to him to think of his doings: but it may be doubted whether this unconsciousness of his greatness ever proved at once more fascinating or more bewildering than when he was discussing scientific results with young men. He so evidently had his wishes centred on a single-hearted desire for the right result that it was difficult to conceive him approaching a question merely as a learner: yet he was ever a learner. There are few men, if any, with not even a tithe of his scientific achievements, who have had less of controversy or have had such immunity from questions as to priority of discovery. This arose not merely from the indisputable

priority of his results : it is partly owing to his nature. Salmon says of him :—

“ His motto has always been ‘esse quam videri,’ and I do not know any one to whom it would be more repulsive to engage in a personal contest by claiming for himself a particle of honour or of money more than was spontaneously conceded. He would be apt to take for his model the patriarch Isaac, who, when the Philistines claimed a well which he had dug, went on and dug another, and when they claimed that, too, went on and dug a third ”:

an exceedingly happy description of the man the tide of whose genius was

“ Too full for sound or foam.”

Some account of his work, some estimate of its character, some indication of the original contributions made by him to his science, may not improperly be given here. It is, of course, impossible to predict what his permanent influence will be upon mathematics, or what opinion coming generations of workers will hold of him : certainly, by his own contemporaries, he was deemed one of the greatest mathematicians the world has seen. Bertrand, Darboux, and Glaisher have compared him to Euler, alike for his range, his analytical power, and, not least, for his prolific production of new views and fertile theories. There is hardly a subject in the whole of pure mathematics at which he has not worked. Some new subjects owe their existence to him ; to others he has made very definite contributions, so that their boundaries have been enlarged often to an enormous extent ; there are few upon which he has not left the mark of his genius.

In several of the notices that appeared at his death he was described as a great explorer. Such he undoubtedly was, but he was more. He not merely discovered new countries but he also opened them up, so that others were able to enter into some possession of those regions without undergoing the difficulties that he had overcome. And if the metaphor may be carried further, he had the restlessness of the explorer : he could not long remain satisfied with an achievement concluded, but must try his fortune again and elsewhere.

Varying opinions have been expressed as to Cayley's style ; the variations are largely due to preconceived views of what a mathematical paper should be. It certainly is not easy to skim one of his papers ; any attempt to do so leads to an inadequate estimate of what it usually establishes. It is not difficult to read one of his papers, even to grasp the contents well, provided proper care be devoted to it, because difficulties that occur are completely solved, and nothing lies

in the background to cause doubt or suggest incompleteness. He has been well described by Glaisher as an unequalled master of analytical processes; it is especially in algebraical manipulation that his strength and his facility stand out in clear view. His success in this direction was achieved by a skill that cannot be explained by describing it as due to acquired knowledge, or to practice, or to long consideration and patient selection. It was rather an instinct for the management of the most complicated processes, and the way in which he controls the most elaborate calculations is sometimes little short of extraordinary.

As regards his methods, he does not seem to have cast about so as to choose one rather than another. As soon as he had thought of any method the possible effectiveness of which he could settle almost intuitively ("one's best things are done in five minutes," he once said to me, in confirmation of the satisfaction I was expressing at the fruitfulness of an idea that had occurred to me unexpectedly), the rest was the exercise of his powers. Among the methods he preferred, especially during the last twenty-five years of his life, was that of verification; in his hands it proved a weapon of great force. Indeed, only less remarkable than his algebraical skill, was the insight which enabled him to preserve the exact equivalence of all the equations in any particular process, so that he could have reversed each process merely by reversing the steps as they were made, and could have proceeded to the required theorem from the initial expression of an algebraical fact. Numerous instances of this quality in his work could be adduced; it will be sufficient to refer to some parts of his paper\* "On the centro-surface of an ellipsoid."

But though Cayley was specially happy in the treatment of algebraical developments, an inadequate estimate of his genius would be obtained by supposing that he was almost entirely an analyst. Much of his thinking, not a little of his writing, is completely geometrical; and his contributions to line geometry, his introduction of the Absolute into geometry, his continued recurrence to the methods in pure geometry invented by Poncelet and Chasles, should be sufficient to range him among geometers.

Moreover, even in strictly analytical work, the synthetic element is often not far away though it does not always appear on the surface. In this connexion an acute suggestion, made by Salmon and perhaps based upon his remembrance of their mathematical correspondence that lasted through many years, is confirmed by one of the Notes Cayley himself added at the end of the second volume of his 'Collected Mathematical Papers.' An enquiry sometimes begins by a comparatively easy problem which, when solved, leads to wider inferences; so

\* 'C. M. P.,' vol. 8, No. 520: 'Camb. Phil. Trans.,' vol. 12 (1873), pp. 319—365.

that, ultimately in the development, considerable generalisations are effected. Now the usual writer, in publishing the results of such an enquiry, draws them up in a sequence that partly marks the order of their connected discovery: and, in doing so, he makes his work easier for his readers. But Cayley was not the usual writer. When he had reached his most advanced generalisations he proceeded to establish them directly by some method or other, though he seldom gave the clue by which they had first been obtained: a proceeding which does not tend to make his papers easy reading. An instance of the fact occurs\* in his 'Memoir on the Theory of Matrices,' where he proves that a matrix satisfies an algebraical equation of its own order; he proves it by verification in simple cases, but he gives no clue as to his line of discovery. An instance of the method occurs in a note† added to one of his papers, where he says that the general equations

$$\{y d_x\} - y d_x = 0, \quad \{x d_y\} - x d_y = 0,$$

characteristic of covariants and invariants of binary quantics were initially suggested by considering the relation of the quadratic  $ax^2 + 2bxy + cy^2$  and its discriminant  $ac - b^2$  to these equations. In the paper he drops linear transformation as connected with the covariant property and defines a covariant as a function satisfying these two equations.

His literary style is direct, simple and clear. His legal training had an influence, not merely upon his mode of arrangement but also upon his expression; the result is that his papers are severe and present a curious contrast to the luxuriant enthusiasm which pervades so many of Sylvester's papers. He used to prepare his work for publication as soon as he had carried his investigations in any subject far enough for his immediate purpose. He found it an easy matter to do this part of his work, and thus differed widely in experience from those to whom the preparation of a paper is laborious even when the results to be incorporated have been obtained. As a matter of fact, he took the straightforward course of saying what he had to say in a clear and simple manner, fixing his mind upon the substance and never going out of his way in order to secure beautiful form for the presentation of results. Yet not infrequently his papers are so admirably written that they satisfy the exacting critics; thus it is perhaps not too much to affirm that his 'Sixth Memoir on Quantics'‡ could not be presented in more attractive form—a character due, however, to the tendency of his method and to his results, but not acquired by any effort specially devoted to elaboration of clear expression. Again, a paper once written out was promptly sent for publication; this practice he

\* 'C. M. P.,' vol. 2, No. 152, pp. 482, 483; 'Phil. Trans.' (1858), pp. 24, 25.

† 'C. M. P.,' vol. 2, p. 600.

‡ 'C. M. P.,' vol. 2, No. 158; 'Phil. Trans.' (1859), pp. 61—90.

maintained throughout his life. He undoubtedly formed projects for the immediate future; thus to the second edition\* of his 'Treatise on Elliptic Functions' he intended to add a couple of chapters which, however, remained unwritten solely for the reason that all such projects were carried into effect only about the time when the need arose. The consequence is that he has left few arrears of unfinished or unpublished papers; his work has been given by himself to the world.

Only one other remark as to the form of his papers need be made. Readers must be struck with the number of exact references he makes to other writers. It was a practice about which he had very decided opinions: he wished not merely to make honourable acknowledgment of indebtedness but also to give indications of the history of the subject. In the latter particular he was always careful to insert in the reference the year in which the book or the paper had appeared; and he steadily urged others to insert dates in their references.

Cayley made additions to every important subject that lies within the range of pure mathematics. Their importance and their amount have varied in different subjects; thus on analytical geometry his writings have a dominating influence: while on the general theory of functions, though he knew the subject well, he has left little mark, for he concerned himself chiefly with details such as the solution of more or less special problems in conformal representation. His papers in general have such value that he is the author most frequently quoted by the great body of current mathematicians. A full record of what he has done in pure mathematics could be made only by writing its history during the last half century; all that is here attempted consists of some brief indications of a selection among his more obviously important contributions to mathematical knowledge.

One of the subjects with which Cayley's name will probably be most closely associated is the theory of invariance. It is easy to cite simple cases of what is implied by an invariantive function: two will suffice.

It is known that, in solving an ordinary algebraical equation with literal coefficients, a certain functional combination of these coefficients (called the discriminant) must vanish in order that two roots of the equation may be equal; for example, the equation  $ax^2+2bx+c=0$ , has equal roots if (and only if) the quantity  $ac-b^2$  vanishes. When the variable is transformed from  $x$  to  $y$  by a relation  $(l'x+m')y=lx+m$ , where  $l, m, l', m'$  are constants, then evidently two values of  $y$ , corresponding to the two equal values of  $x$ , are equal. When  $x$  is eliminated from the equation by means of the assumed relation, a new quadratic arises having  $y$  for its variable; let it be  $a'y^2+2b'y+c'=0$ ,

\* It was published four months after his death; only the earlier sheets had the benefit of his revision.

where  $a', b', c'$  depend upon  $a, b, c$  and  $l, m, l', m'$ . The two values of  $y$  determined by this equation are equal if (and only if) the quantity  $a'c' - b'^2$  vanishes. But the equality of the two values of  $y$  depends upon and is determined by the equality of the two values of  $x$ , the latter equality being secured if the quantity  $ac - b^2$  vanishes. It follows that the vanishing of either of the quantities  $a'c' - b'^2$  and  $ac - b^2$  requires the vanishing of the other; and it is therefore inferred that, when neither of them vanishes, one of them contains the other as a factor. When the actual calculation is made, it is found that  $a'c' - b'^2$  is the product of  $ac - b^2$  and  $(lm' - l'm)^2$ , the latter being a quantity that depends solely upon the transforming relation. Consequently it appears that a combination of the coefficients in the original equation exists, such that when the equation is transformed by any relation of the type indicated and exactly the same combination of the new coefficients is constructed, the two combinations are equal to one another save as to a factor depending solely upon the transforming relation. Such a combination of the coefficients is called an invariant.

Again, it is known that every curve (of degree higher than two) possesses a number of points where a tangent to the curve not merely touches it but, having contact of one degree closer, crosses it; and it is found that all these points, called points of inflexion, also lie upon another curve uniquely derived from the first. When the curves are represented by means of equations, the statement is that the points of inflexion of a curve  $U = 0$  are given as the intersections of this curve with a curve  $H = 0$ , the latter equation being uniquely derived from  $U = 0$ . Now suppose that the axes, to which the curves have been referred, are changed to another system, so that new co-ordinates  $x', y'$  are connected with the former co-ordinates by relations

$$\frac{x'}{a_1x + b_1y + c_1} = \frac{y'}{a_2x + b_2y + c_2} = \frac{1}{ax + by + c}.$$

A new equation  $U' = 0$ , obtained by eliminating  $x$  and  $y$  between these relations and  $U = 0$ , will now represent the curve. The change thus made does not affect the geometrical properties of the curve; its points of inflexion are still given as its intersections with the curve  $H = 0$ . But the points of inflexion of the curve represented by  $U' = 0$  are the intersections of this curve with another curve represented by  $H' = 0$ , an equation derived from  $U' = 0$  in exactly the same way as  $H = 0$  is derived from  $U = 0$ . It therefore appears that the associated curve  $H' = 0$  cuts the given curve in precisely the same points as the associated curve  $H = 0$ , a result which suggests that the associated curves are the same. Now  $H' = 0$  has been derived from  $U' = 0$ ; but actual calculation shows that, if the relations between  $x', y'$  and  $x, y$  be used to eliminate  $x, y$  from  $H = 0$ , the resulting equation is  $H' = 0$ ; in other words, the relations between

$x', y'$  and  $x, y$  transform the equation  $H = 0$ , derived from  $U = 0$ , into the equation  $H' = 0$ , derived in the same way from  $U' = 0$ . Moreover, as in the former case by direct calculation, it is found that  $H'$ , a specially constructed function of  $x', y'$  and the coefficients in  $U'$ , is divisible by  $H$ , the same function of  $x, y$  and the coefficients in  $U$ ; the quotient being a quantity depending again only upon the constants in the transforming relations. Consequently it appears that a combination of the coefficients and the variables in the original equation exists such that, when the equation is transformed by means of relations of the type indicated, and exactly the same combination of the new coefficients and the new variables is constructed, the two combinations are equal to one another save as to a factor dependent solely upon the transforming relations. Such a combination of the coefficients and the variables is called a covariant.

The first notice of such a property appears to have been made by Lagrange. And Gauss discussed the invariance of the discriminants of certain expressions when the latter are subjected to linear transformations. Again, Boole in 1841 had shown that this invariance property belongs to all discriminants, and he gave a method of deducing some other functions of this kind. Boole's paper suggested to Cayley a much more general subject—the permanence of invariance form—so that he set himself the question of finding “all the derivatives of any number of functions which have the property of preserving their form unaltered after any linear transformation of the variables.” The first set of results obtained by his investigations related to invariants; they appeared in his famous paper,\* ‘On the Theory of Linear Transformations,’ published half a century ago. The second set of results related to covariants; they appeared in the paper,† ‘On Linear Transformations,’ published in the succeeding year. In these two papers Cayley demonstrated the general existence of a number of functions, both invariants and covariants (at first he called them hyperdeterminants), which preserve their form under linear transformation.

These discoveries of Cayley establish him as the founder of what is called sometimes modern algebra, sometimes invariants and covariants, sometimes theory of forms; the origination of the theory is incontestably his, and it is universally ascribed to him.

A discovery of this general importance and complete novelty soon attracted the attention of other workers. It is not too much to say that the subsequent investigations long absorbed the active interest of many mathematicians, and, as a result, the theory has influenced all

\* ‘C. M. P.,’ vol. 1, No. 13; ‘Camb. Math. Jour.,’ vol. 4 (1845), pp. 193–209.

† ‘C. M. P.,’ vol. 1, No. 14; ‘Camb. and Dubl. Math. Jour.,’ vol. 1 (1846), pp. 104–122. The two papers were rewritten, and appeared in ‘Crelle,’ vol. 30 (1846), pp. 1–37, under the title “Mémoire sur les Hyperdéterminants.”

that domain of mathematical science which is in any way connected with algebraical form. Among the first to enter the field was Sylvester, then living in London; he and Cayley were in constant communication, alike oral and written, and carried on their work in the most friendly relations with one another. Boole also resumed his investigations, and both he and Salmon made substantial additions to the theory. The continental mathematicians also had begun their important contributions, chief among them being Aronhold, Hesse, and, at a later date, Hermite. Aronhold, indeed, devised the so-called symbolical method, now the favourite method with German workers; in its origin it is nearly the same as the symbolical method introduced by Cayley, but the subsequent developments—due largely also to Clebsch and to Gordan—run on lines entirely different from Cayley's.

After a time, Cayley began his series of ten memoirs on quantics; they must rank among the most wonderful combinations of original researches and papers upon a single theory ever produced. They contain a splendid exposition of the theory as already established; they are full of original contributions to the subject, and as they take account of the work done by other authors, they have the further interest of showing how the subject grew between the appearance of the 'Introductory Memoir' in 1854 and the appearance of the 'Tenth Memoir' in 1878. This is hardly the opportunity to write a history of the subject by apportioning among the various investigators the sections which they respectively originated;\* yet reference should be made to two matters.

First, one of the problems that greatly interested Cayley was the determination of the complete asyzygetic system of irreducible invariants and covariants appertaining to a binary form, that is, the system such that every invariant and every covariant of the form can be expressed as a rational integral algebraical function of the members of the system, the coefficients in the function being numerical only. In his 'Second Memoir on Quantics'† he had accurately determined the number (and their degrees) of the asyzygetic invariants for binary forms of orders 2, 3, 4, 5, 6; he had also accurately inferred the number (together with their degrees and their orders) of the asyzygetic covariants for binary forms of orders 2, 3, 4, all these concomitants being subsequently tabulated. But, in

\* Some information will be found in an appendix to Salmon's 'Lessons on Higher Algebra;' also in the notes and references at the end of the second volume (pp. 598—601) of the 'Collected Mathematical Papers.' A valuable and exhaustive report, containing a full history of the subject, was drawn up by Prof. Dr. Franz Meyer, and published under the title "Bericht über den gegenwärtigen Stand der Invariantentheorie" ('Jahresber. d. Deutschen Mathem.-Vereinigung,' I, 1892).

† 'C. M. P.,' vol. 2, No. 250; 'Phil. Trans.' (1856), pp. 101—126.

regard to the invariants of forms of order higher than 6 and the covariants of forms of order higher than 4, he came to the erroneous conclusion that the respective numbers are infinite. The error was not corrected until Gordan in his memoir,\* dated 8th June, 1868, and entitled 'Beweis dass jede Covariante und Invariante einer binären Form eine ganze Function mit numerischen Coefficienten einer endlichen Anzahl solcher Formen ist,' showed that the complete system for a binary quantic of any order contains only a limited number of members. Cayley at once returned to the question, and having found a source of error (it was the neglected interdependence of certain syzygies, reducing the numbers of invariants and covariants; the interdependence had not previously been suspected), he dedicated his 'Ninth Memoir on Quantics'† (dated 7th April, 1870), to the correction of the error and a further development of the theory in the light of Gordan's results. His promptness in recognising and giving immediate prominence to the work of the younger author possibly prevented some controversy among unwise partisans; it was characteristic of the man.

And, secondly, though his series of memoirs was brought to an end with the tenth, his interest in the subject did not cease, and he frequently wrote upon parts of it under other titles. In particular, Captain P. A. MacMahon's discovery of a relation of a new character between seminvariants and symmetric functions (viz., that the leading coefficients of the covariants of a binary quantic are the same as the non-unitary partition symmetric functions of the roots of an equation connected with a modified quantic) proved of the keenest satisfaction to him. From time to time he wrote in the 'American Journal of Mathematics' upon this subject and upon symmetric functions generally in this connection, always sympathetic and appreciative of the advances made by others, able to grasp and assimilate their ideas, but using them as a master and not as a follower. It was not alone, however, to symmetric functions, upon which he had written long and important memoirs as early as 1857, but to many other cognate subjects that he extended his researches upon invariants and covariants. The theory of equations of the fifth and higher degrees, Sturm's functions, Tschirnhausen's transformation, partition of numbers, Arbogast's method of derivation, skew determinants‡—to quote no others—are titles and subjects of papers, in all of which are investigations of great value. The reason that they are less known (if such be the case) than his other work in the same line of ideas is perhaps due to the fact that the direct theory of in-

\* 'Crelle,' vol. 69 (1869), pp. 323—354.

† 'C. M. P.,' vol. 7, No. 462; 'Phil. Trans.' (1871), pp. 17—50.

‡ His discoveries in this subject alone have done much to simplify the analytical investigations connected with Pfaff's problem and the allied theory.

variants and covariants was rapidly brought within the range of students through Salmon's 'Lessons on Higher Algebra,' dedicated by the author to Cayley and Sylvester.

Another subject, of which he must be regarded as the creator, is the theory of matrices. His first memoir\* upon this theory, "wherein," to quote Sylvester,† "he may be said to have laid the foundation stone of multiple quantity," was published in 1858. A couple of isolated results had been obtained by Hamilton in 1852 through the methods of quaternions; but they were unknown to Cayley at the time of his memoir, and, owing to the connection in which they occur, they have an entirely detached aspect.

A matrix may initially be defined as a symbol of linear operation; thus, when the equations

$$X = ax + by + cz, \quad Y = a'x + b'y + c'z, \quad Z = a''x + b''y + c''z$$

are expressed in the form

$$(X, Y, Z) = \begin{pmatrix} a, & b, & c \\ a', & b', & c' \\ a'', & b'', & c'' \end{pmatrix} \begin{matrix} X \\ Y \\ Z \end{matrix} = M(x, y, z),$$

the symbol  $M$  is a matrix. Cayley was the first to discuss the theory of such symbols as subjects of functional operation and to dispense with the hitherto regular return at each stage to the equations of substitution in which the symbol first arises; in fact, he replaces the notion of substitutional operation by the notion of a new class of quantity.

Matrices (being of the same order or dimension) can be added like ordinary algebraical quantities; as regards multiplication, they are subject to the associative law, but not to the commutative law. Hence powers of a matrix (positive and negative, integral and fractional) can be obtained, and likewise algebraical functions of a matrix. It also follows that two general matrices are not convertible, that is,  $LM$  is not the same as  $ML$  save under special conditions, and it is a part of the theory to find the most general matrix convertible with a given matrix. The expression of this convertible matrix can be deduced by means of the fundamental equation which every matrix satisfies, viz., an algebraical equation of its own order, the coefficient of the highest term being unity, and the last term being the determinant of the constants in the matrix. All these results were given by Cayley in his initial memoir; and, at the same time, they were applied by him to obtain the most general automorphic linear transformation of a bipartite quadric function, a problem which is the generalisation of that which requires the most general (orthogonal)

\* 'C. M. P.,' vol. 2, No. 152; 'Phil Trans.' (1858), pp. 17—37.

† 'Amer. Journ. Math.,' vol. 6 (1884), p. 271.

substitution transforming the function  $x^3+y^3+z^3+\dots$  into the function  $x'^3+y'^3+z'^3+\dots$ .

How fruitful the subject has proved may be inferred by noting the subsequent investigations of Sylvester, who has developed it on Cayley's lines, and has added to it many new ideas; of Tait, who developed the theory of quaternions on parallel lines: of the Peirces, father and son, whose researches on linear associate algebra\* gave rise to the notion of matrices from a different source; of Clifford and Buchheim, who connected the theory with Grassmann's methods; of Laguerre, in whose memoir† the treatment of a "linear system" (the same as a Cayley matrix) is similar to Cayley's; and of many other writers, among whom Taber should be mentioned.

Connected with non-commutative algebraical quantities, Cayley's researches on the theory of groups require a passing notice. He devoted several papers to questions in this theory. Some of them relate to those groups of substitutions, the introduction of which by Galois made an epoch in the theory of equations, others of them relate to groups of homographic transformations, particularly those related to the polyhedral functions. But, so far as can be seen, he limited his published investigations to those groups which are finite and discontinuous.

Abstract geometry—the ideal geometry of  $n$  dimensions—is a subject that he may almost be said to have created; no other name than his has been associated with its origin. More than anything else, it marks the line of difference between the kinds of homage accorded to him. Experts regard it as an illustration of his imaginative power: the unlearned regard it as an incomprehensible mystery.

It finds a place among his earliest investigations,‡ it was steadily present to his mind, illuminating many of his researches; and occasionally it found explicit treatment, e.g., in his 'Memoir on Abstract Geometry,'§ and in his Presidential Address at Southport. The theory presents itself in two connections: one, as a need in analysis, the other as a generalisation of the ordinary geometries of two dimensions and of three dimensions.

The former origin can be indicated in a brief statement. When an occasion arises for dealing with a number of variables, connected in any manner and regarded as either variable or determinate (wholly or partially), the nature of the relations among them is frequently indicated, and often is made more easily intelligible, by associating some geometrical interpretation with the given system of relations.

\* 'Amer. Journ. Math.,' vol. 4 (1881), pp. 97–220.

† "Sur le Calcul des Systèmes Linéaires" ('Journal de l'Éc. Poly.,' t. 25, 1867, pp. 215–264).

‡ 'C. M. P.,' vol. 1, No. 11; 'Camb. Math. Journ.,' vol. 4 (1845), pp. 119–127.

§ 'C. M. P.,' vol. 6, No. 413; 'Phil. Trans.' (1870), pp. 51–63.

Thus the momental ellipsoid is of great use in the discussion of moments of inertia, in representing the motion of a body round a fixed point when there are no impressed forces, and in other questions in dynamics. Again, two non-homogeneous (or three homogeneous) variables can be regarded as the co-ordinates of a point in a two-dimensional geometry, such as that of a plane or the surface of a sphere or any analytical surface; and any equation among the co-ordinates is then interpreted as representing a curve (or curves, or portion of a curve or curves) upon the surface. Similarly, when there are three non-homogeneous (or four homogeneous) variables, they can be regarded as the co-ordinates of a point in a three-dimensional geometry, such as that of ordinary space; corresponding to an equation among the variables, there is a surface (or surfaces) in space; corresponding to two independent equations among the variables, there is a curve (or curves) in space; and corresponding to three independent equations, there is a point (or points) in space. In such cases the analytical relations can often, with great advantage, be exhibited as geometrical properties. When the number of non-homogeneous co-ordinates is greater than three (or the number of homogeneous co-ordinates is greater than four), the circumstances have greater need of such a representation, while there is a greater difficulty in constructing some geometrical illustration; and then it can be obtained in a corresponding form only by the idea of a space of the proper number of dimensions. To secure the possibility of such a representation, it is necessary to evolve the geometry of multiple space.

For example, there are four single theta-functions, and their squares are connected by linear homogeneous relations. In order to obtain other properties of the functions themselves, it is convenient to regard them as homogeneous co-ordinates of a point in (ordinary) space; the amplitude in space that then is to be selected is the quadri-quadric tortuous curve represented by those linear relations, viz., the curve which is common to two quadric cylinders with intersecting axes. Similarly there are sixteen double theta-functions, with corresponding linear relations among their squares. The associated geometry is fifteen-dimensional; the manifoldness in this space to be selected for the discussion of the properties is the quadri-quadric two-dimensional amplitude common to thirteen quadric hyper-cylinders.

An initial difficulty in the construction of an analytical geometry of  $n$ -dimensions is the expression of an amplitude of less than  $n-1$  dimensions by means of equations that shall represent the complete amplitude, and nothing besides the amplitude. It occurs in ordinary solid geometry, the difficulty there being to obtain the expression of a tortuous curve in space by means of equations that represent it alone. For instance, a twisted cubic is frequently taken as the inter-

section of two quadrics having one common generator; but the equation of the quadrics taken together represent not the cubic curve alone but also the common generator. And the like for other cases.

Cayley's purpose in his 'Memoir on Abstract Geometry,' already referred to, was the exposition of some of the elementary principles of the subject. The paper is a remarkable instance of his power of presentation of abstract ideas, and of his clear precision of statement. Moreover, he makes it an explanatory paper; and, in view of the prevailing estimate of him as an analyst, it is worthy of notice that the paper does not contain a single equation, and contains only a few symbols. It is unnecessary to summarize its contents; the furthest stage reached is the establishment of the notion that underlies the principle of duality in geometry.

But though the necessity for hyperdimensional geometry can thus be met so far as it arises in connection with analysis, it is a different matter when the geometry is to be regarded as the generalisation of the geometries of two-dimensional space and of three-dimensional space. Cayley's reply to his own question as to the meaning to be attached to hyperdimensional space is\* that

"It may be at once admitted that we cannot conceive of a fourth dimension of space; that space as we conceive of it, and the physical space of our experience, are alike three-dimensional; but we can, I think, conceive of space as being two- or even one-dimensional; we can imagine rational beings living in a one-dimensional space (a line) or in a two-dimensional space (a surface), and conceiving of space accordingly, and to whom, therefore, a two-dimensional space, or (as the case may be) a three-dimensional space would be as inconceivable as a four-dimensional space is to us."

By not a few people the first clause in this passage has been neglected and the later clauses have not always been read rightly; and his further remark, "I need hardly say that the first step is the difficulty, and that granting a fourth dimension we may assume as many more dimensions as we please," has left some readers rather puzzled as to whether Cayley had not, after all, some mysterious incommunicable conception of a fourth dimension. His position is stated in the first clause of the former passage: his conclusion is that hypergeometry is, and is only, a branch of mathematics.

Before passing from the consideration of his larger contributions to hypergeometry, it is proper to mention his introduction of the six co-ordinates of a line. These are six quantities connected by a homogeneous equation  $af + bg + ch = 0$ ; and as only their ratios are used, they are thus equivalent to only four independent magnitudes, suffi-

\* 'Brit. Ass. Report,' 1883, President's Address, p. 9.

cient for the unique specification of a right line. They were first established, and primarily used by him, in connection with his new analytical representation of curves in space;\* and he often recurred to the subject, devoting in particular one paper† to the calculus of the six co-ordinates and to a discussion of Sylvester's involution of six lines. It should, however, be stated that these co-ordinates presented themselves independently to Plücker; the development of Plücker's theory as set forth in his memoir‡ 'On a New Geometry of Space,' and in his book§ 'Neue Geometrie des Raumes,' is entirely different from that obtained by Cayley, and it ought to be regarded as a separate creation. And it need hardly be remarked that while the introduction of a line, as an entity represented by a set of co-ordinates, leads to a new geometry of space, it is also clear that line-geometry can be regarded as a geometry of four dimensions.

Another notion, entirely due to Cayley in its first form, is that of the Absolute; it was first introduced in his 'Sixth Memoir on Quantics,'|| which was devoted chiefly to his investigations on the generalised theory of metrical geometry.

It is a known property that the angle between two lines AB, AC, when multiplied by  $2\sqrt{-1}$ , is equal to the logarithm of the cross-ratio of the pencil made up of the lines AB, AC, and (conjugate imaginary) lines joining A to the circular points at infinity; and the measure of the angle between two lines can thus be replaced by the consideration of a projective property of an extended system of lines. Other examples of similar changes could easily be quoted. The purpose of Cayley's theory was to replace metrical properties of a figure or figures by projective properties of an extended system composed of the given figure or figures and of an added figure.

But it is not solely owing to the generalisation of distance that the memoir is famous. It has revolutionised the theory of the so-called non-Euclidian geometry; and it has important bearings on the logical and philosophical analysis of the axioms of space-intuition. The independence and the importance of the ideas, originated by Cayley in this memoir, have never been questioned; but, as is often (and naturally) the case with the discoverer of a fertile subject, Cayley himself did not explain or foresee the full range of application of his new ideas. He did not recognise, at the time when his memoir was first published, the beautiful identification of his generalised theory of metrical geometry with the non-Euclidian geometry of Lobatchewsky and Bolyai. This fundamental step was taken by Klein in his

\* 'C. M. P.,' vol. 4, Nos. 284, 294.

† 'C. M. P.,' vol. 7, No. 435.

‡ 'Phil. Trans.,' 1865, pp. 725—791.

§ Leipzig, Teubner, 1868.

|| 'C. M. P.,' vol. 2, No. 158; 'Phil. Trans.' (1859), pp. 61—90.

admirable memoir\* 'Ueber die sogenannte Nicht-Euklidische Geometrie,' which contains a considerable simplification in statement of Cayley's original point of view, and contributes one of the most important results of the whole theory. The work of the two mathematicians now being an organic whole, there is no advantage—at least here—in attempting to subdivide the subject for the purpose of specifying the exact share of each in its construction.

The scope of the Cayley-Klein ideas may briefly be gathered from the following sketch. Let  $A_1$  and  $A_2$  be two points, often called a point-pair; they are to be either both real or, if not both real, then conjugate imaginaries so far as their co-ordinates are concerned. Let  $P, Q, R$  be three other points on the line  $A_1 A_2$ ; and let the symbol  $(PQ)$  denote

$$2\gamma \log \frac{A_1P \cdot A_2Q}{A_1Q \cdot A_2P} \text{ or } 2i\gamma \log \frac{A_1P \cdot A_2Q}{A_1Q \cdot A_2P},$$

according as  $A_1$  and  $A_2$  are a real point-pair, or an imaginary point-pair. Then it is manifest that

$$(PQ) + (QR) = (PR),$$

so that the functions  $(PQ), (QR), (PR)$  satisfy the fundamental property of the distances between  $P$  and  $Q, Q$  and  $R$ , and  $P$  and  $R$ . Consequently  $(PQ)$  may be taken as a generalised conception of the distance between the points  $P$  and  $Q$ .

Now let a conic be described in a plane, either imaginary, say, of the form  $x^2 + y^2 + z^2 = 0$  or real, say, of the form  $x^2 + y^2 - z^2 = 0$ . Choosing the latter case, let attention be confined to points lying within the conic, so that every straight line through a point cuts the conic in a real point-pair. Take two points,  $P$  and  $Q$ ; and let the line joining them cut the conic in two points,  $A_1$  and  $A_2$ . Then  $(PQ)$ , as defined above (the constant  $\gamma$  being the same for all such lines), is the generalised distance between  $P$  and  $Q$ . This conic, which has been arbitrarily assumed, and upon which the generalised conception of distance depends, is termed by Cayley the Absolute.

Cayley, however, avoided the unsatisfactory procedure of using one conception of distance to define a more general conception. As he himself explains more fully,† he regarded the co-ordinates of points as some quantities which define the relative properties of points, considered without any reference to the idea of distance but conceived as ordered elements of a manifold. Thus if  $\alpha_1, \beta_1, \gamma_1$  and  $\alpha_2, \beta_2, \gamma_2$  be the co-ordinates of the point-pair  $A_1$  and  $A_2$ , the co-ordinates of the points

\* 'Math. Ann.,' vol. 4 (1871), pp. 573—625.

† See the note which he added, 'C. M. P.,' vol. 2, p. 604, to the Sixth Memoir; it contains some interesting historical and critical remarks.

P and Q on the line  $A_1A_2$  can be taken as  $\lambda_1\alpha_1 + \lambda_2\alpha_2$ ,  $\lambda_1\beta_1 + \lambda_2\beta_2$ ,  $\lambda_1\gamma_1 + \lambda_2\gamma_2$  and  $\mu_1\alpha_1 + \mu_2\alpha_2$ ,  $\mu_1\beta_1 + \mu_2\beta_2$ ,  $\mu_1\gamma_1 + \mu_2\gamma_2$  respectively. The function (PQ) can then be defined as

$$2\gamma \log \frac{\lambda_2\mu_1}{\lambda_1\mu_2} \text{ or } 2i\gamma \log \frac{\lambda_2\mu_1}{\lambda_1\mu_2};$$

the generalised idea of distance thus finds its definition without any antecedent use of the conception in its ordinary form. Cayley's view is summed up in his sentence\* :—" . . . the theory in effect is, that the metrical properties of a figure are not the properties of the figure considered *per se* apart from everything else, but its properties when considered in connection with another figure, viz. the conic termed the absolute."

The metrical formulæ obtained when the absolute is real are identical with those of Lobatchewsky's and Bolyai's "hyperbolic" geometry: when the absolute is imaginary the formulæ are identical with those of Riemann's "elliptic" geometry; the limiting case between the two being that of ordinary Euclidian ("parabolic") geometry.

Cayley's memoir leads inevitably to the question, as to how far projective geometry can be defined in terms of space perception without the introduction of distance. This has been discussed by von Staudt† (in 1847, previous to Cayley's memoir), by Klein‡ and by Lindemann.§ The memoir thus points to a division of our space intuitions into two distinct parts: one, the more fundamental as not involving the idea of distance, the other, the more artificial as adding the idea of distance to the former. The consideration of the relation of these ideas to the philosophical account of space has not yet been brought to its ultimate issue.

It is in analytical geometry, both of curves and of surfaces, that the greatest variety of Cayley's contributions is to be found. There is hardly an important question in the whole range of either subject in the solution of which he has not had some share; and there are many properties our acquaintance with which is due chiefly, if not entirely, to him. How widely he has advanced the boundaries of knowledge in analytical geometry can be inferred even from the amount of his researches already incorporated in treatises such as those by Salmon, Clebsch and Frost; and yet they represent only a portion of what he has done. In these circumstances only a selection among his con-

\* *Loc. cit.*, § 230.

† 'Geometrie der Lage,' also in his later 'Beiträge zur Geometrie der Lage,' 1857.

‡ 'Math. Ann.,' t. 6 (1873), pp. 112—145.

§ 'Vorlesungen über Geometrie' (Clebsch-Lindemann), vol. 2, Part I; the third section is devoted to the subject.

tributions can be indicated: it must be understood that, here as elsewhere, the statement does not pretend to be a complete account.

It is an old-established property that two curves of degrees  $m$  and  $n$  cut in  $mn$  points, but that it is not possible to draw a curve of degree  $n$  through any  $mn$  arbitrarily selected points on a curve of degree  $m$ . As early as 1843, Cayley extended the property and showed that when a curve of degree  $r$  higher than either  $m$  or  $n$  is to be drawn through the  $mn$  points common to the two curves, they do not count for  $mn$  conditions in its determination, but only for a number of conditions smaller than  $mn$  by  $\frac{1}{2}(m+n-r-1)(m+n-r-2)$ . A single addition was made to the theorem by Bacharach\* in 1886—taking account of the case when the undetermining points lie on a curve of degree  $m+n-r-3$ ; with this exception the algebraical problem was completely solved by Cayley in his original paper.† The result is often called Cayley's intersection-theorem.

Another geometrical research of fundamental importance was embodied by him in a memoir‡ 'On the higher singularities of a plane curve,' published in 1866: it is there proved that any singularity whatever on a plane algebraical curve can be reckoned as equivalent to a definite number of the simple singularities constituted by the node, the ordinary cusp, the double tangent and the ordinary inflexional tangent. The theory has, since that date, been developed on lines different from Cayley's—owing to its importance in other theories, such as Abelian functions, variety in its development has proved both necessary and useful; but it was Cayley's investigations in continuation of Plücker's theory that have cleared the path for the later work of others.

The classification of cubic curves had been effected by Newton in his tract 'Enumeratio linearum tertii ordinis,' published in 1704: and six species had been added by Stirling and Cramer, the total then being 78. Plücker effected a new classification in his 'System der analytischen Geometrie,' published in 1835: his total number of species is 219, the division into species being more detailed than Newton's. Cayley re-examined the subject in his memoir§ 'On the classification of cubic curves,' expounding the principles of the two classifications and bringing them into comparison with one another; and entering into discussion with full minuteness, he obtains the exact relation of the two classifications to one another—a result of great value in the theory.

To the theories of rational transformation and correspondence he made considerable additions. Two figures are said to be rationally

\* 'Math. Ann.,' vol. 26 (1886), pp. 275—299.

† 'C. M. P.,' vol. 1, No. 5; 'Camb. Math. Journ.,' vol. 3 (1843), pp. 211—213.

‡ 'C. M. P.,' vol. 5, No. 374; 'Quart. Math. Journ.,' vol. 7 (1866), pp. 212—223

§ 'C. M. P.,' vol. 5, No. 350; 'Camb. Phil. Trans.,' vol. 11 (1864), pp. 81—123.

transformable into one another when to a variable point of one of them corresponds reciprocally one (and only one) variable point of the other. The figure may be a space or it may be a locus in a space. Rational transformations between two spaces give rational transformations between loci in those spaces; but it is not in general true that rational transformations between two loci necessarily give rational transformations between the spaces in which those loci exist. There is thus a distinction between the theory of transformation of spaces and the theory of correspondence of loci. Both theories have occupied many investigators, the latter in particular; and Cayley's work may fairly be claimed to have added much to the knowledge of the theory as due\* to Riemann, Cremona and others.

Further, there may be singled out for special mention, his investigations on the bitangents of plane curves and, in particular, on the 28 bitangents of a non-singular quartic; his developments of Plücker's conception of foci; his discussion of the osculating conics of curves, and of the sextactic points on a plane curve (these are the places where a conic can be drawn through six consecutive points); his contributions to the geometrical theory of the invariants and covariants of plane curves; and his memoirs on systems of curves subjected to specified conditions. Moreover, he was fond of making models and of constructing apparatus intended for the mechanical description of curves. The latter finds record in various of his papers; even so lately as 1893 he exhibited, at a meeting of the Cambridge Philosophical Society, a curve-tracing mechanism connected with three-bar motion.

All the preceding results belong to plane geometry; no less important or less numerous were the results he contributed to solid geometry. The twenty-seven lines that lie upon a cubic surface were first announced in his memoir† 'On the Triple Tangent Planes of Surfaces of the Third Order,' published in 1849, after a correspondence between Salmon and himself. Cayley devised a new method for the analytical expression of curves in space by introducing into the representation the cone passing through the curve and having its vertex at an arbitrary point. Again, by using Plücker's equations that connect the ordinary (simple) singularities of plane curves, he deduced equations connecting the ordinary (simple) singularities of

\* In this connection a report by Brill and Noether, "Bericht über die Entwicklung der Theorie der algebraischen Functionen in älterer und neuerer Zeit" ('Jahresber. d. Deutschen Mathem.-Vereinigung,' vol. 3, 1894) will be found—particularly the sixth and the tenth sections—to give a very valuable *résumé* of the theory and its history.

† 'C. M. P.,' vol. 1, No. 76; 'Camb. and Dubl. Math. Journal,' vol. 4 (1849), pp. 118–132. See also Salmon's 'Solid Geometry' (third edition, 1874), p. 464, note.

the developable surface that is generated by the osculating plane of a given tortuous curve, and, therefore, also of any developable surface. He greatly extended Salmon's theory of reciprocal surfaces; and, resuming a subject already discussed by Schläfli, he produced\* in 1869 his 'Memoir on Cubic Surfaces,' in which he dealt with their complete classification. Many of his memoirs are devoted to the theory of skew ruled surfaces, or scrolls as he called them. Our knowledge of geodesics, of orthogonal systems of surfaces, of the centro-surface of an ellipsoid, of the wave-surface, of the 16-nodal quartic surface, not to mention more, is due in part to the extensions he achieved. It is difficult to indicate parts of the general theory of surfaces and of twisted curves that do not owe at least something and frequently much to his labours; a mere reference to the index of a book like Salmon's 'Solid Geometry' will show how vast has been his influence.

One group of subjects interested him throughout his life, the theory of periodic functions, in particular, of elliptic functions: it was to the latter that his only book was devoted. But in a subject, the main lines of which were established so definitely before he began to write,† it is impossible, without entering into great detail, to mark out the contributions that are directly due to him. When a theory is in such a stage as was that of elliptic functions about 1842, the work of one writer sometimes helps to fill the gaps left by that of another, sometimes develops another writer's results from a different point of view; the composite theory depends, in part, upon the coordination of complementary results.

Abel's famous paper,‡ 'Mémoire sur une propriété générale d'une classe très-étendue de fonctions transcendentes,' presented to the French Academy of Sciences in 1826, and unfortunately delayed in publication§ for nearly fifteen years, attracted Cayley's attention quite early in his scientific career. In 1845 Cayley published his 'Mémoire sur les fonctions doublement périodiques,'|| in which he considered Abel's doubly-infinite products of the form

$$u(x) = x \prod \left(1 + \frac{x}{w}\right),$$

\* 'C. M. P.,' vol. 6, p. 412; 'Phil. Trans.' (1869), pp. 231—326.

† The history will be found in Casorati, 'Teorica delle funzioni di variabili complesse,' 1868, and in Enneper, 'Elliptische Functionen, Theorie und Geschichte,' second edition, 1890, where other references are given.

‡ 'Œuvres complètes d'Abel' (Christiania, 1881), vol. 1, pp. 145—211.

§ The circumstances are recited in § 9 of the appendix to the volume, by Bjerknes, 'Niels Henrik Abel, Tableau de sa vie et de son action scientifique' (Gauthier-Villars, Paris, 1885).

|| 'C. M. P.,' vol. 1, No. 25; 'Lionville,' t. 10 (1845), pp. 385—420.

where  $w = (m, n) = m\Omega + nY$ , the ratio  $\Omega : Y$  is not real, and the product is taken for all positive and all negative integer values of  $m$  and of  $n$  between positive and negative infinity, except simultaneous zero values. He showed that such products can be used to obtain Jacobi's elliptic functions by constructing fractions such as

$$u(x + \tfrac{1}{2}\Omega) \div u(x);$$

and he also showed that the actual value of any product involves an exponential factor  $e^{Bx^2}$ , where the value of the constant  $B$  depends upon the relation\* between the infinities of  $m$  and of  $n$ . The results were of definite importance at the time of their discovery, and they still hold their place. But the form of the doubly-infinite product has been modified† by Weierstrass, who takes

$$\sigma(x) = x \prod \left\{ \left( 1 + \frac{x}{w} \right) e^{-\frac{x}{w} + \frac{x^2}{w^2}} \right\},$$

a function the value of which is independent of any particular form of relation between the infinities of  $m$  and of  $n$ . Owing to the latter simplification, Cayley's results are, as he himself remarked,‡ partly superseded by those of Weierstrass.

Cayley had great admiration for the works of both Abel and Jacobi; he had begun to read the latter's 'Fundamenta Nova' immediately after his degree. The prominent position occupied in that work by the theory of transformation naturally attracted his interest; and, even as early as 1844 and 1846, he wrote short memoirs upon the subject, obtaining in one of them a function, due to Abel and now known as the octahedral function. Further memoirs of a similar tenor appeared occasionally; they deal chiefly with transformation as concerned with the known differential relation of the form

$$\{(1-x^2)(1-k^2x^2)\}^{-\frac{1}{2}}dx = M\{(1-y^2)(1-\lambda^2y^2)\}^{-\frac{1}{2}}dy.$$

The contributions made to the transformation theory by Sohnke, Joubert, and Hermite, as well as Jacobi's original investigations, all depend upon the use of transcendental functions of the quantity

\* This is sometimes expressed differently, as follows. Points are taken having  $m$  and  $n$  for their Cartesian co-ordinates; those which occur for infinite values of  $m$  and of  $n$  lie at infinity, and may be considered to lie upon a curve altogether at infinity, the shape of which is determined by the relation between the infinities of  $m$  and of  $n$ .

The value of the constant  $B$  is said to depend upon the shape of this bounding curve.

† Weierstrass's investigations on infinite products are contained in his memoir "Zur Theorie der eindeutigen analytischen Functionen" ('*Abh. d. K. Akad. d. Wiss. zu Berlin*,' 1876); also in his book '*Abhandlungen aus der Functionenlehre*,' 1896.

‡ '*C. M. P.*,' vol. 1, p. 536.

$q (= e^{-\frac{\pi'}{k}})$ : yet the results are such that they ought to be deducible by ordinary algebraical processes. It was Cayley's wish to deal with this theory by pure algebra; two simple cases had already thus been discussed by Jacobi, but the extension to the less simple cases proved difficult. Cayley's 'Memoir on the transformation of elliptic functions,'\* carries on the algebraical theory and places it in a clearer light than before. But though he made a distinct advance in dealing with particular cases, he still found it necessary to use the  $q$ -transcendents for making any definite advance in the general case. And the same compulsion occurs in the chapters of his 'Treatise' (dated 1876), where transformation is discussed at considerable length.

He resumed his investigations in 1886, still dealing with the algebraical method, but applying it to a simplified form of elliptic integral due to Briochi. Though the problem is not solved† completely for the general case, he has devised a method which is effective at least in part; it easily leads to new results connected with the modular equations in the known simpler cases previously solved.

The theta-functions are the subject of several of his papers. He began‡ with a direct establishment of Jacobi's relation

$$\sqrt{k} \operatorname{sn} u = H(u) \div \Theta(u),$$

obtained in the 'Fundamenta Nova' by a long and cumbrous process; and he proceeded to the construction of the linear differential equations satisfied by the theta-functions. Except, however, in so far as they arise in the transformation theory, they do not appear to have occupied him until about 1877. In that year and in the succeeding years he wrote a number of papers dealing with the theta-functions on an independent basis and not as a detail in elliptic functions. Though the investigations are concerned with  $p$ -tuple functions, yet, partly for simplicity, and partly in order to secure the greater detailed development of the theory, the papers deal chiefly with the cases  $p = 1, p = 2$ .

Previous to Cayley's investigations, the most valuable algebraical results in this subject were those of Rosenhain§ and Göpel,|| which had connected the double theta-functions with the theory of the

\* 'C. M. P.,' vol. 9, No. 577; 'Phil. Trans.,' 1874, pp. 397—456.

† The memoirs of this period belonging to the transformation of elliptic functions were published in the 'American Journal of Mathematics,' vol. 9 (1887), pp. 193—224; vol. 10 (1888), pp. 71—93.

‡ "On the Theory of Elliptic Functions," 'C. M. P.,' vol. 1, No. 45; 'Camb. and Dubl. Math. Journal,' vol. 2 (1847), pp. 256—266.

§ 'Mém. des Savans Étrangers,' t. 11 (1851), pp. 361—468; the paper is dated 1846.

|| 'Crelle,' vol. 35 (1847), pp. 277—312.

Abelian functions of two variables, and those of Weierstrass, developed by Königsberger\* to give the "addition-theorem." Proceeding in his 'Memoir on the Single and Double Theta-functions'† more by Göpel's method than by Rosenhain's, Cayley resumes the whole theory. He pays special attention to the relations among the squares of the functions and to the derivation of the biquadratic relation among four of the functions, which is the same as the equation of Kummer's sixteen-nodal quartic surface. To this relation and to the geometry of this associated surface he frequently recurred, both specifically in isolated papers and generally in researches upon quartic surfaces.

As connected, in part, with elliptic functions, his investigations on the porism of the in- and circumscribed polygon should be mentioned. The porismatic property of two conics, viz., that they may be related to each other so that one polygon (and, if one polygon, then an infinite number of polygons) can be inscribed in one and circumscribed about the other, is due to the geometrician Poncelet. The special case when the conics are two circles had been discussed analytically by Jacobi,‡ using elliptic functions for the purpose. Cayley undertook, first in 1853, the analytical discussion of the most general case of two conics, also using elliptic functions; and he obtained§ the relations, necessary for the porism, for the several polygons as far as the enneagon. And it may be remarked, as a characteristic instance of Cayley's habit of proceeding to general cases, that he did not leave the matter at this stage. In a memoir|| 'On the Problem of the in- and circumscribed Triangle' he raises the question as to the number of polygons which are such that their angular points lie on a given curve or given curves of any order and their sides touch another given curve or given curves of any class. Using the theory of correspondence, he solves the question completely in the case of a triangle—taking account of the fifty-two cases that arise through the possibility of two curves, or more than two curves, being one and the same curve.

From time to time Cayley turned his attention to questions in theoretical dynamics, choosing them as subjects of his lectures during his earlier years as professor. Among them may be mentioned his investigations on attractions, specially those on the attraction of ellipsoids, to which he devotes five memoirs,¶ discussing the methods of Legendre, Jacobi, Gauss, Laplace, and Rodrigues; and his

\* 'Crelle,' vol. 64 (1865), pp. 17—42.

† 'Phil. Trans.,' 1880, pp. 897—1002.

‡ 'Ges. Werke,' t. 1, pp. 277—293; this paper was published first in 'Crelle,' t. 3 (1828), pp. 376—389.

§ In a set of five papers, 'C. M. P.,' vol. 2, Nos. 113, 115, 116, 128; *ibid.* vol. 4, No. 267.

|| 'C. M. P.,' vol. 8, No. 514; 'Phil. Trans.' (1871), pp. 369—412.

¶ 'C. M. P.,' vol. 1, Nos. 75, 89; vol. 2, Nos. 164, 173, 193.

evaluations or reductions of multiple definite integrals connected with attractions and potentials in general, particularly his 'Memoir on Prepotentials,'\* in which he discusses the reduction of the most general integral of the type that can occur in dealing with the potential-problem related to hyperspace. He also frequently recurred at intervals, before drawing up his report about to be quoted, to the consideration of the motion of rotation of a solid body about a fixed point under no forces. By introducing Rodrigues's co-ordinates into the equations of motion he was able to reduce the solution of the problem to quadratures; but the final solution of this case, in the most elegant form, is due to Jacobi himself; it involves single theta-functions. It may be remarked that the next substantial advance made in the theory of motion of a body under the action of forces is due to the late Madame Sophie Kovalewsky, who, in a memoir† to which the Bordin Prize of 1888 was awarded by the Paris Academy of Sciences, has shown that the motion can, in a particular case, be determined in terms of double theta-functions when the body rotating round a fixed point is subject to the force of gravity.

Sometimes, after reading widely upon a subject, Cayley would draw up a report recounting the chief researches in it made by the great writers. It occasionally happens in the development of a theory that periods come when the incorporation and the marshalling of created ideas seem almost necessary preliminaries to further progress. Cayley was admirably fitted for work of this kind, owing not only to his faculty of clear and concise exposition, but also to his wide and accurate knowledge. Among such reports, two are of particular importance; his 'Report on the recent progress of theoretical dynamics'‡ and his 'Report on the progress of the solution of certain special problems of dynamics'§ have proved of signal service to other writers and to students. His knowledge and his power of summarising are shown also in some interesting articles on mathematical topics, written by him for the 'Encyclopædia Britannica.'

Cayley also had a great enthusiasm for some of the branches of physical astronomy. Some idea of the value and importance of his labours in this subject, particularly in connection with the development of the disturbing function in both the lunar theory and the planetary theory, and with the general developments of the functions that arise in elliptic motion, may be gathered by consulting the series of memoirs|| which he communicated to the Royal Astronomical Society.

\* 'Phil. Trans.,' 1875, pp. 675—774.

† 'Mém. des Savants Étrang.,' t. 31 (1894), No. 1.

‡ 'C. M. P.,' vol. 3, No. 195; 'Brit. Ass. Report' (1857), pp. 1—42.

§ 'C. M. P.,' vol. 4, No. 298; 'Brit. Ass. Report' (1862), pp. 184—252.

|| They are included, with very few exceptions, in the third and the seventh volumes of the 'Collected Mathematical Papers.'

Special reference should be made to one of Cayley's astronomical papers. In 1853 Adams had made a new investigation of the value of the secular acceleration of the moon's mean motion, and, taking account of the variation in the eccentricity of the earth's orbit, had obtained a value which differed from that given by Laplace. Unfortunately, Adams's result was disputed by some of the great school of French physical astronomers, notably by Pontécoulant, and, in consequence, some hesitation about acceptance was felt by some English astronomers, perhaps not unnaturally in view of the severe criticisms expressed. Cayley made an independent investigation of the necessary approximations, and devised a new method for introducing the variation of the eccentricity in question—a method effective perhaps chiefly owing to the instinct and power with which he carried out the laborious analysis required. The memoir, in which he embodied his results and which was entitled 'On the secular acceleration of the moon's mean motion,'\* completely confirmed the value obtained by Adams, and was of substantial help in settling the controversy.

And, in the last place, the preceding sketch of Cayley's contributions to mathematical science seems to refer, for the most part, only to long memoirs. Yet it must not therefore be supposed that his shorter papers (which are very numerous) can safely be neglected. Sometimes he wrote a simple note not so much to convey new results as to set out his view of some particular theorem; these notes were always fresh and often suggestive. He was specially gratified when he had obtained a brief solution of some question, and his quite short papers frequently contain most important results. For instance, in the brief paper† 'On the theory of the singular solutions of differential equations of the first order,' he was the first to give a clear exposition of the theory which in Boole's book had been left in an imperfect state. He there obtained the broad essential results of the theory, and it is particularly on his work, and on the work of Darboux published very soon after Cayley's, that ulterior researches are based.

What has been said may be sufficient to point out Cayley's place among the mathematicians of his time, and to indicate the services he rendered to the science which he loved so well. But he was more than a mathematician. With a singleness of aim, which Wordsworth could have chosen for his 'Happy Warrior,' he persevered to the last in his nobly lived ideal. His life had a significant influence on those who knew him: they admired his character as much as they respected his genius: and they felt that, at his death, a great man had passed from the world.

A. R. F.

1st June, 1895.

\* 'C. M. P.,' vol. 3, No. 221; 'Monthly Not. R.A.S.,' vol. 22 (1862), pp. 171—231.

† 'C. M. P.,' vol. 8, No. 545; 'Messenger of Math.,' vol. 2 (1873), pp. 6—12.

IN SIR HENRY CRESWICKE RAWLINSON, Bart., G.C.B., England has lost one of her most brilliant Oriental scholars, and one of her most distinguished Anglo-Indian statesmen. He was born in the year 1810, being the second son of Mr. Abram Tyssack Rawlinson, of Chadlington, Oxfordshire, who was descended from an old Lancashire family, members of which have represented both that county and the borough of Liverpool in Parliament. Having passed his school-days at Wrington, Somerset, and at Ealing, he joined the service of the East India Company, at Bombay, in 1827, being then about seventeen years of age. He at once devoted himself with much energy to the study of Oriental languages, and within a year of his arrival in India was appointed interpreter to the 1st Bombay Grenadiers. While serving in Bombay and Poonah he acquired great proficiency both in Persian and Mahrattée, and in 1833 he was one of a number of British officers sent by the Governor-General of India to aid the Persian Government in reorganizing its army. It was here that he first became acquainted with those archæological remains to which he ever after devoted so much attention. A rupture with Persia led to his being withdrawn from that country, which he left in 1838. He played an important part in the Afghan war, which soon followed, having been appointed political agent at Kandahar in 1840. At the conclusion of the war in 1843, Rawlinson became British Consul for Bagdad, and subsequently Consul-General, an office which enabled him to return to the study of those Persian and Assyrian antiquities with which he had been fascinated. In 1856 he returned to England and was made a K.C.B. and a Director of the East India Company on the nomination of the Crown. In 1858 he became a member of the Council of India, and in the following year was sent as envoy to the Court of Teheran, a post he occupied for about a year. Of his subsequent short career in Parliament and of his political views with regard to the encroachments of Russia in the East, it is hardly necessary to speak.

In recognition of his incessant labours and valuable discoveries in connexion with Assyrian archæology he was elected a Fellow of the Royal Society in 1850. It was indeed in that year that his "Commentary on the Cuneiform Inscriptions of Babylonia and Assyria" was published.

Since that time 45 years have passed; but it is now 60 years since Rawlinson, in 1835, began to make copies of the inscribed tablets of Elwend, so that he had already laboured 15 years in what was then a field of absolute mystery. When once a science has become thoroughly developed, we are always too apt to forget the work of those who laid the foundations on which others have built, and whatever may have been the labours of Grotefend, Norris, Hincks, Westergaard, or Lassen, it is to Rawlinson that the merit is

due of having been the first to read and make connected sense of the inscriptions. Lassen had found out the values of most of the Persian signs, and Grotefend laid down a plan; but Rawlinson put both together and added the values of the missing signs from analogous forms in Zend, Pehlevi, and Sanskrit. By immense personal exertions,\* and at no small risk, he was able to obtain transcripts of the inscriptions engraved on the precipitous rock of Behistun at a height of 500 feet above the plain, and having mastered the old Persian character, he obtained a key by which he was enabled to unlock many of the secrets of Persian history, while the Babylonian version of the Behistun text, together with the newly-discovered syllabaries and sign-lists from Nineveh, enabled him to bring to light many historical facts from the Assyrian inscriptions at Khorsabad and Nineveh (Kouyunjik).

Some misunderstanding has existed about Rawlinson's share in the decipherment both of Persian and Semitic cuneiform texts, and it may be as well here to recall a few of the more important facts in connexion with his work. There is abundant evidence that he was working at the texts of Elwend so far back as 1835, and in 1836 he had mastered all that Grotefend had made out, even succeeding in correcting some of the results obtained by him in 1804. The short inscriptions of Elwend were, however, insufficient for Rawlinson's purpose, and so he boldly attacked the long text inscribed on the rock at Behistun; and at the end of 1836 he had translated several passages in it. In 1837 he began to correspond with the Royal Asiatic Society with a view of sending them a translation of the whole text; in 1838 he sent in a general statement of his discovery, and in 1839 his complete manuscript was ready for despatch. It will be remembered that Lassen published his work on the Ancient Persian cuneiform texts in 1836, and there have not been wanting those who asserted that without it Rawlinson could never have obtained his excellent results, inasmuch as he was indebted to it for the phonetic values of many of the Persian cuneiform characters. This is not the place to adduce minute proofs of the original character of Rawlinson's work, but it may be stated as a well-known fact that the discovery of several phonetic values was peculiarly his own, and that by applying other values to the cuneiform characters in question than his own, he could not have translated the Behistun text. Moreover, Lassen's book did not come into Rawlinson's hands until two years after his translation had been made and sent in to the Royal Asiatic Society. As soon as political troubles broke out in Persia in

\* An idea of the difficulties encountered in this work may be obtained from the fact that MM. Coste and Flandin, who were sent out by the French Government to copy the Behistun text, returned empty-handed and reported the rock to be inaccessible.

1839-40 Rawlinson's studies stopped perforce, and he was unable to renew them until he took up his residence as British Consul in Bagdad early in 1844. Soon after, the printing of his translation of the Behistun text and commentary began, and two years later the work appeared. When we have awarded all due praise to all other labourers in the field of Persian cuneiform research, the fact remains that Rawlinson was the first man to translate into a European language two hundred lines of Persian cuneiform and to give a correct grammatical analysis and commentary. Three years later, *i.e.*, in 1849, he produced a translation of the Babylonian or Semitic section of the Behistun text, which was printed in 1851. In this marvellous work he gave a list of about 250 cuneiform characters with their phonetic and ideographic values.

Another triumph of this distinguished man ought not to be forgotten. When the Trustees of the British Museum commissioned him to publish copies of the Assyrian cuneiform texts in the British Museum, the late Mr. Fox Talbot suggested that the great inscription of Tiglath-Pileser I. should be translated by more than one cuneiform scholar in order that a comparison of the translations might show if experts had arrived at any well-grounded system of interpretation which was known and accepted by them generally. The Royal Asiatic Society asked Rawlinson, Hincks, Oppert, and Fox Talbot to send in translations under sealed covers which were to be opened simultaneously by a committee of examination which included Dean Milman, Dr. Whewell, Sir G. Wilkinson, Mr. Grote, Mr. W. Cureton, and Prof. H. H. Wilson. After the specified time (one month) the translations were sent in, and the Committee found that Rawlinson and Fox Talbot had translated the whole text, Hincks one half, and Oppert rather less, and that the general agreement of the translations was such as to leave no doubt in the minds of the examiners that the interpretation of cuneiform now rested on a thoroughly scientific basis, which, it may be added, was founded chiefly by Rawlinson.

In 1851, by means of a grant from the British Museum, he superintended systematic explorations at Kouyunjik, with the aid of Mr. Hormuzd Rassam, and many valuable relics were obtained, which are now in the British Museum. The site is still under the control of the Trustees of the Museum, and it is to be regretted that from financial causes further investigations cannot now be properly carried on. The five volumes of "The Cuneiform Inscriptions of Western Asia," which, with the successive assistance of Messrs. Edward Norris, George Smith, and Theophilus Pinches, he prepared for the Trustees between 1861 and 1884, testify to the value of these excavations and to the untiring energy of Sir Henry Rawlinson. In 1876 he was elected a Trustee of the British Museum, and was on all occasions ready to promote the interests of that institution in every possible manner.

Of his independent works, principally of travel in the East, and of his numerous contributions to the Journals of the Royal Asiatic and Geographical Societies, of the former of which he was for many years Director, and of the latter President, it seems needless to speak. His merits as a discoverer were recognized by honorary degrees conferred upon him by the Universities of Oxford, Cambridge, and Edinburgh, by the Prussian Order *pour le Mérite*, and by his foreign membership of the Institute of France, *Académie des Inscriptions*. It is to him and to the late Sir Henry Layard that this country is indebted for the most extensive and important collection of the historic remains of the ancient kingdoms of the East that has ever been brought together.

J. E.

VALENTINE BALL, LL.D., C.B., the distinguished Director of the National Museum at Dublin, whose death we have to deplore, was the second son and fourth child of the well-known naturalist, Dr. Robert Ball, who died in 1857. His elder brother is Sir Robert Ball, now of Cambridge, and his younger brother is Dr. Charles B. Ball, of Merrion Square, Dublin. Dr. Valentine Ball was born on July 14, 1843, at No. 3, Granby Row, Dublin, a house well known in those days as a leading centre of intellectual resort in that metropolis. He was educated first at a private school by Dr. Brindley, at Chester, and afterwards by Rev. Dr. Benson, in the early days of Rathmines School. Valentine Ball entered Trinity College in 1860, and about the same time he was appointed by the late Master Fitzgibbon to a clerkship in the office of the Examiner in Chancery. His University career was not an eventful one in the academic sense, for the duties of his office in the Four Courts did not leave him sufficient time for more than obtaining an ordinary degree. A taste for scientific pursuits was, however, so marked, that in 1864, when he was twenty-one years of age, he was offered a position in the Geological Survey of India, then under the direction of one of his father's oldest friends, Dr. Thomas Oldham. Ball felt that this would give him the opportunity which he wanted for the study of Nature in a wide field, and accordingly he went to India. His duties as a geological surveyor often led him into very unfrequented parts of our great Oriental possessions, and frequently, for many months together, he lived in camp in the jungle, apart from all other Europeans. Wherever Ball travelled he utilised his opportunities to the utmost; indeed, throughout his life, his diligence could hardly have been surpassed, and nothing worthy of notice that came within his range was unobserved and unrecorded. It was presently apparent that the young geological surveyor was not only able to fulfil his duties in making a careful investigation of the rocks and of their economic value, but that various other branches of natural

history were simultaneously sedulously cultivated by him. He became an authority on the birds of India, he wrote upon the various jungle products, he devoted much attention to anthropology; see, for example, his study of the Songhal and other native races. He recorded their racial peculiarities, and discussed their affinities, he made himself acquainted with their languages; paper after paper flowed from his ready pen, describing well and describing accurately aspects of our Indian Empire which but few, indeed, have had both the opportunity of observing and the capacity of recording. Steadily the reputation of the Indian geologist advanced in scientific circles. He was elected a fellow of the Calcutta University in 1872. He devoted a short vacation to extending his travels to the Andaman and Nicobar Islands, and there noted what seemed interesting and novel, and at every opportunity he was increasing the collection of objects which illustrated his studies in anthropology and natural history. His first important volume, 'Jungle Life in India,' gives a record of his travels and summarises the results of his multitudinous papers. This work was followed by an elaborate treatise on the economic geology of India. His fame had by this time become so firmly established that on the resignation of the Chair of Geology in the University of Dublin by Rev. Dr. Haughton, Valentine Ball was appointed his successor. Thus was brought to a close his connexion of seventeen years with the Geological Survey of India. Summoned thus to a post of dignity and influence as a Professor in his own University in his native city, Ball threw himself with characteristic ardour into the discharge of his new duties. He attracted around him at the outset a band of diligent students, and in 1882 became a fellow of the Royal Society. The Professor's intimate acquaintance with Indian geology led to his being frequently consulted by the Indian Government on various scientific or economic questions. He was, indeed, on one occasion requested to visit and report upon the Burmese ruby mines, but he had to decline on account of his other duties. Much of his knowledge on the practical aspect of Indian geology was published in his book on the 'Diamonds and Gold of India.' His valuable services were recognised by the Indian Government, and they awarded him a special pension, notwithstanding that his residence in India fell short by several years of what would ordinarily have entitled him to such a privilege. But the time was approaching when he was to be called to the important position in which his chief life-work was to be accomplished. In September, 1883, Ball was appointed to the post of Director of the new Science and Art Museum about to be established in Dublin. He accordingly surrendered his professorship of geology and mineralogy in the University of Dublin after only two years' tenure. He has placed on record that the reason for accepting the directorship and relinquish-

ing a post of much greater ease and freedom of action was mainly this, that the management of a museum of wide range had been the dream of his life, and the occasions when he had had temporary charge of museums in India, including that of the Imperial Museum in Calcutta, only whetted his long cherished desire for a permanent appointment of the kind. The new Director devoted himself with much vigour to the great work of developing the Museum, as well as the affiliated institutions of the National Library, the Botanic Gardens, Glasnevin, and the School of Art. It would be impossible in this place to chronicle fully the growth of the collection under the Director's charge. Suffice it to say that there has been a steady influx of most valuable objects to the spacious halls in Kildare Street. We need only mention, for instance, two collections each absolutely unique of its kind. First of all there are the Irish antiquities, including the famous gold ornaments from the Royal Irish Academy, while the more recent important acquisition has been due to the kindness of the authorities of Trinity College, who have deposited there a superb collection of weapons and implements, brought, in part at least, from the South Sea Islands by Captain Cook. In the collection of the multitudinous objects now brought together in the Museum, and in the arrangements of their display, the late Director, assisted as he was by a diligent and skilful staff, took the liveliest and most absorbing interest.

Though his death was premature, yet it may be said that he lived long enough to see the substantial completion of his life's task. The arrangement of the new Museum will remain as a testimony to his work.

The University of Dublin conferred on him the honorary degree of Doctor of Law, and by Her Majesty he was made a Companion of the Bath. With most of the scientific societies of Dublin Dr. V. Ball was in intimate association; especially will he be remembered in connexion with the Royal Zoological Society, of which he was the assiduous Secretary for so many years, and of which the welfare was so dear to him.

In 1879, Dr. Ball married the eldest daughter of the late John Stewart Moore, of Moyarget, Co. Antrim. He leaves a family of four young children. For some years Dr. Ball's health had been failing. Towards the middle of June serious symptoms were manifested, and he passed away peacefully on the afternoon of June 15, at his residence, 28, Waterloo Road, Dublin.

R. S. B.

JOHN WHITAKER HULKE was born in the year 1830, and died on Tuesday, the 19th of February, 1895. The immediate cause of his death was broncho-pneumonia following influenza, and apparently

caused by a chill taken when attending, in the early morning, a serious case at the Middlesex Hospital. Mr. Hulke was the son of a well-known surgeon of Deal, in which town his family had resided for several generations, and where his mortal remains are now laid to rest. Mr. Hulke received his early education at a private boarding school, where it appears he was very unhappy, and he was therefore quite ready to appreciate the kindness which, notwithstanding the roughness of the school life, he experienced at the Moravian College at Neuwied, where his education was continued from 1843 to 1845; it was here that he gained his intimate knowledge of the German language, and the groundwork of his acquaintance with natural history; here also, in the Eifel district, his interest in geology was first awakened.

Returning to England, he studied at King's College School during 1846 and 1847, entering the medical department of that college in 1849, where for the following few years he underwent his medical training.

In 1855 he was attached to the medical staff of the General Hospital in the Crimea, and in March of that year was doing duty in the English Hospital at Smyrna. Here the medical officers appear to have had comparatively comfortable quarters, but often very few patients. Some excitement was kept up by a band of brigands roaming the neighbourhood, and on one occasion, a doctor of the town, having been carried off by them in the hope of a ransom, Mr. Hulke was among the first to start to the rescue. In September he left Smyrna for the camp before Sebastopol in the hope of gaining more experience, and here, during the winter of 1855-56, owing to the severe climate and other causes, he had a very trying time, on more than one occasion narrowly escaping the shots of the Russians, but, as he himself has said, gaining in those few months years of experience. Letters sent home to Deal at this time contain graphic accounts of his surroundings, but no word of complaint of the hardships undergone.

On returning to England he became a Fellow of the Royal College of Surgeons, and received the appointment of Assistant-Surgeon to the Royal Ophthalmic Hospital, Moorfields, in 1857. His well-known and classical essay on 'Diseases of the Retina' was written soon afterwards, and was awarded the Jacksonian Prize of the Royal College of Surgeons in 1859. Not long after this he published a treatise on the ophthalmoscope, an instrument in use in Germany, but at that time not known to English practitioners. Besides other works relating to the diseases of the eye, Mr. Hulke made many contributions to general surgery, which were published in the 'Medico-chirurgical Transactions,' and elsewhere.

In the year 1862 Mr. Hulke was appointed Surgeon to the Middle-

sex Hospital, and it was here that the chief of his life's work may be said to have been accomplished. His skill as an operator became widely known, while the keen interest which he took in his patients, and his kindly sympathy with them were best appreciated by those who in their trouble and sickness were so fortunate as to be under his care. He eventually became senior surgeon to this hospital, a post he retained until the time of his death.

In 1876 he was appointed Examiner to the Royal College of Surgeons, and in 1880 became a member of the Court of Examiners; in 1881 he was elected a member of the Council, in 1888, a Vice-President, and in 1893 President, which latter position he held at the time of his death. In 1883 he was President of both the Pathological and Geological Societies.

Mr. Hulke was admitted to the Fellowship of the Royal Society in 1867, his claim being based exclusively on researches relating to the anatomy and physiology of the retina in man and the lower animals, particularly the reptiles. These were embodied in two papers in the 'Philosophical Transactions' ("On the Anatomy of the *Fovea centralis* of the Human Retina," and "On the Chameleon's Retina"), and in a paper on the "Retina of Amphibia and Reptiles," in the first volume of the 'Journal of Anatomy and Physiology.' These are characterised by patient and conscientious minuteness in the working out and description of details and cautious reserve in drawing inferences. Probably the most important and permanently valuable of Mr. Hulke's researches was the one relating to the retina of the chameleon, which the abundant material at his disposal enabled him to elaborate in a more complete manner than had before been possible.

In judging of the value of histological work done nearly a generation ago, it must be remembered that at that time the minute anatomist had to work alone. Hulke was the first in this country to follow in the footsteps of Max Schultze and Heinrich Müller—the first to employ those, at that time, new methods of research which have rendered it possible to acquire that relatively perfect knowledge of the fine structure of the organs of special sense which we now possess. If the work of Hulke and his contemporaries is unknown, as no doubt it is to the student of the present day, it is not because it was unimportant, but rather because the anatomical facts then made out for the first time with very imperfect means of investigation, have been presented to him in sharper outline by men who, after all, only built on the foundations laid by their predecessors. Hulke very soon after he became a Fellow of the Royal Society transferred his allegiance to geology, between which and his profession as a consulting surgeon his energies were thenceforth to be divided. Had he continued his anatomical studies he would without doubt have attained to the foremost rank among physiological anatomists.

During the quarter of a century which followed his first contributions to geological science, Mr. Hulke found leisure to apply himself to research in this field, notwithstanding his constantly increasing practice. He did so to so good a purpose that he became a palæontologist of no ordinary merit. His knowledge of comparative anatomy, and especially of osteology, enabled him rapidly to grasp the meaning of structures presented by the remains of fossil vertebrates; and this, combined with a naturally keen perception and a rigid adherence to facts, soon caused his opinion on palæontological matters to be sought, and held in the highest estimation.

It was the fossil Reptilia which more especially occupied Mr. Hulke's attention, and his numerous papers on their osteology are a monument to his industry. Many of the fossils which he described were, in part at least, freed from the matrix by his own facile chisel; and in this mechanical work, as he himself has said, he often found relaxation when his mind was over-wrought by professional anxieties.

Mr. Hulke's well-earned vacations were often spent at localities of geological interest, more especially with a view to working out the fossils which might be obtained. For this purpose he paid many visits to Brook, in the Isle of Wight, from whence have come many specimens of Wealden Dinosauria; near here also, at Brixton, was preserved the unique collection of these Wealden reptiles, made by the Rev. W. Fox. For many years Mr. Hulke was the only palæontologist who had free access to this collection; and he did much good work in bringing to light its hidden treasures, which otherwise remained almost unknown until after the death of their owner, when they were transferred to the British Museum.

In the year 1868 Mr. Hulke was elected a Fellow of the Geological Society of London, and from that time onwards the pages of the Quarterly Journal of that Society were frequently enriched by his writings. No fewer than six of his papers were published in the two volumes which followed the year of his election, and these with one exception were descriptions of Saurian remains from the Kimmeridge clay of Dorset. Several other papers on reptiles from the same locality appeared in subsequent volumes; but Mr. Hulke was more particularly interested in the Dinosauria, and many contributions to the osteology of this interesting group of reptiles have appeared in the 'Quarterly Journal' of the Geological Society, and in the 'Philosophical Transactions' of the Royal Society.

Our first knowledge of the cranium of *Iguanodon* was due to Mr. Hulke's work upon a specimen from the Isle of Wight, which did not include the facial bones, and the affinities of which it was by no means easy to determine.

In 1873 and 1874 he made additions to our knowledge of the small Wealden Dinosaur, which had been named by Professor Huxley

*Hypsilophodon Fozii*; and in 1882 a still more important memoir on the same species was published in the 'Philosophical Transactions.'

In 1874 and 1876 Mr. Hulke showed that a certain bone of *Iguanodon*, which had been regarded as a scapula, was really a part of the pelvis; and, indeed, it proved to be the remarkable pubis of that reptile, which so nearly resembles that of a bird.

In 1879 the two genera, *Poikilopleuron* and *Megalosaurus*, were shown by Mr. Hulke to be one and the same Dinosaurian genus. In the same year he described the remains of a new Wealden Dinosaur under the name of *Vectisaurus Valdensis*; and in 1880 he made known one of the most perfect *Iguanodons* discovered in this country which had been found in the Kimmeridge clay of Cumnor; this was named *Iguanodon Prestwichii*.

In the following year there appeared in the 'Philosophical Transactions' Mr. Hulke's memoir on *Polacanthus Fozii*. This remarkable Dinosaur, the name for which had been suggested by Sir R. Owen, has a large dermal shield spread out above the iliac bones in such a way as to form a kind of carapace over the lumbar and sacral regions; besides this, large spines and scutes were attached to other parts of the body.

Mr. Hulke's presidential addresses to the Geological Society, 1883-4, formed an important contribution to our knowledge of reptilian osteology, and especially threw light on the structure of the shoulder girdle in Plesiosaurs and their allies.

The *Iguanodont* remains found in England have been more or less fragmentary, and discoveries made by other workers helping to elucidate their structure were hailed by Mr. Hulke with satisfaction; no one more heartily rejoiced than he did when the rich treasures of the Belgian Wealden rocks were brought to light by the geologists of that country, and they made known the discovery of the series of magnificently perfect *Iguanodon* skeletons, several of which now adorn the Museum of Natural History at Brussels.

Mr. Hulke was for many years on the Council of the Geological Society, and the high esteem in which he was held by the leading geologists of the day, as well as their thorough appreciation of his palaeontological work, found expression by their electing him, in 1882, to fill the Presidential chair of their Society, and, in 1887, by their presenting him with the Wollaston Gold Medal, the greatest honours it was in their power to bestow. In 1891 he was elected Foreign Secretary of the Geological Society, which office he still held at the time of his decease.

Beloved and respected by all who knew him, Mr. Hulke will long be lamented as a Christian gentleman.

J. B. S.

E. T. N.

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HENRY JOHN CARTER was born at Budleigh Salterton, Devonshire, on the 18th August, 1813. He studied medicine from the age of sixteen, when he entered the Devon and Exeter Hospital as an indoor pupil, and in 1835 he became a student at University College, where he gained two silver medals for Comparative and Human Anatomy. In 1839, after passing his examination at the College of Surgeons, he was appointed House Surgeon at University College, but shortly afterwards he gave up this appointment on account of a temporary failure of health, and became Conservator of the College Museum. In 1841 he studied in Paris, at the École de Médecine, and in the following year received the appointment of Surgeon, in the service of the East India Company.

Carter's Indian career began on the 12th February, 1842, when he landed in Bombay, and terminated in 1862. In the earlier portion of the twenty years he was engaged on military duty, in the course of which he was dispatched from Bombay to Calcutta by sea, and he returned by way of Mauritius, encountering a cyclone on each voyage. Soon after returning he was placed in medical charge of a battery of artillery that was ordered to join the army in Sind, under Sir C. Napier, and he was present at the battle of Hyderabad, on March 24th, 1843, for which he received the medal. Later in the year he was sent with a detachment of troops to Umarmkot, and subsequently he proceeded in medical charge of H.M.'s 21st Regiment, to Nasirpur, in pursuit of Shah Mahomed. These expeditions, carried out in the Indian desert, at a very hot season of the year, entailed great sickness and mortality amongst the troops, and severe exertions on the part of the medical staff. When, towards the close of 1843, Carter returned with the 21st Regiment to Karachi, nearly every man was on the sick list.

His next service was of a pleasanter description and may be regarded as virtually the commencement of his scientific career. In 1844 he was appointed Medical Officer of the surveying brig, "Palmirus," then commanded by Captain Sanders, I.N., and for the next two years he remained with the vessel during the survey of the South East Coast of Arabia, from the entrance of the Persian Gulf, to the Straits of Bab-el-Mandeb, at the southern extremity of the Red Sea. The geological notes made by Carter, during these two years, were published in the Journal of the Bombay Branch of the Royal Asiatic Society, in 1852, and it may be remarked that even to the present day but few additions of importance have been made to them; for our knowledge of the Eocene and Cretaceous rocks, and of the associated igneous formations of Southern Arabia, we have still in great measure to depend upon Carter's account.

From July, 1846, when he was appointed Assistant Civil Surgeon in Bombay, the remaining sixteen years spent by him in India were

passed in the capital of the Western Presidency. Here he soon became a scientific leader amongst the small body of men, mostly belonging to the medical staff of the Presidency, who were interested in the geology of the country, and here he published a series of papers, many of them of considerable length, on various geological and biological subjects. The different offices filled by him in turn amply testify to his energy and to the esteem in which he was held. From 1847 till he finally left India, he was Honorary Secretary to the Bombay Branch of the Royal Asiatic Society, having previously been for a short time Hon. Sec. of the Medical and Physical Society. In 1851 he was Hon. Sec. to the Bombay Committee of the Great Exhibition in London, in 1854 he occupied a similar post in relation to the Paris Exhibition of that year; in 1859 he presided over a Committee appointed by the Government to establish an Economic Museum, and in the same year he was elected President of the Medical and Physical Society, was appointed a Fellow of Bombay University, and a Justice of the Peace for Bombay. On his retirement, in 1862, the Bombay Branch of the Royal Asiatic Society presented him with £100 for the purchase of a microscope in recognition of his services to the Society during the fifteen years that he had held the office of Honorary Secretary.

On his retirement from India, with the rank of Surgeon-Major, Carter settled in his native place, Budleigh Salterton, and in 1864 he married an Irish lady, who, with an only child, a daughter, survives him. This marriage was eminently conducive to the happiness of his declining years, passed amidst the quiet surroundings of his early boyhood. On October 4th, 1888, he suffered from a paralytic attack, which impaired his powers of speech and his eyesight, and caused him to relinquish scientific and literary work, though he continued for more than six years afterwards to enjoy the society of a few intimate friends. In the spring of the present year his strength declined seriously, and on the evening of May 4th, he passed quietly away in sleep.

Although he was not the author of any large original work, the list of Carter's papers in the Society's catalogue to 1873 comprises 169 entries, a number that affords abundant evidence of his scientific industry, and that was greatly increased before his death, whilst the subjects of the papers testify to considerable versatility. The complete list of his contributions to science is classified by Mr. W. Theobald, his friend and executor, from whose notes most of the details here given are taken, under the following heads:—Ethnology, 4; medicine, 13; geology, 19; zoology and botany (exclusive of the two next categories), 47; foraminifera, 28; sponges, 127; total, 238. The best known of his earlier contributions to science, published

in India, were geological and palæontological, though the index to his original papers and compilations, published in 1862 ('*Jour. Bom. Br. R.A.S.*,' vi, p. 231) shows that even then his contributions to zoology, botany, and physiology, were more numerous than his geological writings. His best known and most important papers, besides that already noticed on the South-East Coast of Arabia, were on the geology of Bombay and the neighbouring islands; on the Fossil Foraminifera of Sind, Cutch, &c., and, above all, the 'Summary of the Geology of India between the Ganges, the Indus, and Cape Comorin.' In some cases, as in the 'Geology of Bombay Island,' later observers have seen reasons for coming to conclusions differing from those of Carter; and his admirable attempt, in the 'Summary of the Geology of India,' to classify the rock-formations of the Indian Peninsula, suffered from the disadvantage that not only had he had no opportunity of seeing the rocks of the greater part of the country, but he had to depend on the descriptions of local observers of varying geological powers, each acquainted with but a comparatively small area. But no writer better deserves the respect and gratitude of Indian geologists, or has contributed more effectively to the advancement of their knowledge. His especial service to the science consisted in his being for many years a centre of geological energy, in his obtaining from many local observers and thus rescuing from oblivion notes on various parts of Western India that have greatly facilitated later systematic work, and in his selection and publication, as editor for the Government of Bombay, of the collected 'Geological Papers on Western India' in 1857. The writer of the present notice can testify to the advantage he repeatedly derived from Carter's careful and conscientious compilation.

But it is upon his researches amongst sponges and foraminifera that Carter's scientific fame is mainly based. It was to the structure and classification of these, and more particularly of the sponges, that he devoted himself after his return to England in 1862, and his papers on various members of the groups appeared uninterruptedly, chiefly in the *Annals and Magazine of Natural History*, from before the date of his retirement from India to within a few years of his death. His last paper, which appeared in 1889, was a "Sketch of the History of known Fossil Sponges in relation to those of the present day." For many years he was occupied with the description and classification of the sponges in the National Collection, his aid in the work having been enlisted by the late Dr. J. E. Gray. Amongst other subjects he wrote on Eozoon, and was one of the first to support the views of King and Rowney as to the inorganic nature of that problematical fossil. He was a skilful microscopist and his ability as a draughtsman enabled him to record his microscopical observations with fidelity. Some years before his death he

presented his private collection of sponges and foraminifera to the British Museum.

Surgeon-Major Carter became a Fellow of the Society in 1859, and in 1872 received a Royal Medal "for his long-continued and valuable researches in zoology, and more especially for his enquiries into the natural history of the *Spongiadae*." He was a corresponding member of the Academy of Natural Sciences, Philadelphia, and of the Boston Society of Natural History.

W. T. B.

JAMES DWIGHT DANA was born on February 12, 1813, at Utica, New York, U.S.A., and in his 83rd year died suddenly, from heart failure, at New Haven, Connecticut, on April 14, 1895. He received his early education at Bartlett Academy, Utica, while there showing great interest in chemical experiments, and making frequent excursions in search of minerals; to the training received at this school he ascribed much of the success attained by him in after life. In 1833, attracted by the reputation of Professor Benjamin Silliman, he went to reside at New Haven, and entered at Yale College, where he not only studied classics and mathematics, but made much progress in the natural sciences, especially in mineralogy and botany. His mathematical distinction led to his appointment as instructor of mathematics to the midshipmen of the United States Navy, and in that official capacity he left New Haven, in 1833, to cruise in the Mediterranean. A visit made to Vesuvius in 1834, during this term of office, led to the publication of his first paper. In 1836 he returned to New Haven, and stayed there two years, acting for the greater part of that time as assistant to Professor Silliman. In 1837 he published the first edition of his *Descriptive Mineralogy* (580 pages). In the following year he was appointed mineralogist and geologist to the United States expedition, which sailed, under Charles Wilkes as commander, on an exploring voyage round the world. The expedition consisted of two sloops-of-war, a store ship, and a brig; the voyage extended over four years (1838—1842), and the scientific staff included, in addition to Dana, as mineralogist and geologist, Pickering, Couthouy and Peale as zoologists, Rich and Brackenridge as botanists, and Hale as philologist. On the return home of one of his colleagues, Dana further took upon himself the charge of the crustacea and zoophytes.

The study of the material collected by the expedition and the preparation of his reports occupied all the available time during the next 13 years; the first two or three years of this period were spent at Washington, but after his marriage, in 1844, to Henrietta Frances, third daughter of Professor Silliman, he thenceforward lived at New Haven, and was closely associated in his work with Professor Silliman,

until the death of the latter, in 1864. In 1846 he became an associate of Professor Silliman in the editing of the *American Journal of Science*, and after the death of his father-in-law, became the principal editor of that important scientific serial. In addition to his original papers, upwards of 200 in number, he wrote for that journal hundreds of abstracts, critical reviews, obituary notices, and notes on many topics. In 1850 he was appointed Professor of Geology and Natural History at Yale College. In 1864, on an increase of the professoriate, his duties as professor were restricted to geology and mineralogy. In 1892 he formally retired, and last year was made Professor Emeritus.

Professor Dana's studies were thus of a manifold character, and as a consequence he achieved distinction in various branches of science—mineralogy, geology, and zoology. In the former subject his chief work was his *Descriptive Mineralogy*, the first edition of which was published at his own expense in 1837 (580 pages), and of which the sixth edition—published in 1892, under the superintendence of his distinguished son, Professor Edward Salisbury Dana—maintains the high reputation for completeness and accuracy attained by the previous editions through which this standard work has passed. At the time of the issue of the first edition, the natural history method of classification of minerals was employed by many mineralogists. At first Dana adopted that system, and went so far as to devise a systematic nomenclature on the dual Latin plan; but in the third edition (1850), the whole system—classes, orders, genera, Latin names, and all—was rejected, and the importance of chemical composition was recognised. In the preparation of this work Dana spared no pains, and to aid in the settlement of the vexed question of mineral names, made an exhaustive review of the entire literature of mineralogical science. He wrote about forty separate papers on purely mineralogical subjects; many are descriptive of particular mineral species, but most of them treat of general subjects, such as nomenclature, pseudomorphism, homœomorphism, and the connection between crystalline form and chemical constitution.

In geology his work was equally extensive. In 1862 he brought out the first edition of his excellent and well illustrated *Manual of Geology*, having special regard to the geology of the North American continent. A fourth edition of this work (1087 pages) was issued only two or three months before his death. Each edition of this manual, like those of his *Descriptive Mineralogy*, was characterised by carefulness and thoroughness of treatment, the last edition being completely re-written from beginning to end. As illustrations of the variety and philosophical character of his geological studies, we may cite his papers on the origin of the grand outline features of the earth, the origin of continents, mountains, and prairies, the early conditions

of the earth's surface, the analogies between the modern igneous rocks and the so-called primary formations, on erosion, on denudation in the Pacific, on terraces, and the formation of valleys. Further, he was an active field geologist, and more especially studied the so-called Taconic system, chiefly in Western New England, and the glacial phenomena of Southern New England. The region included in Western Connecticut and Massachusetts, and extending westward into New York, and north to Vermont, was traversed many times, and there was scarcely an outcrop in the whole of this difficult region which was not visited by him, and carefully examined and described. His geological report for the Wilkes expedition occupies 756 pages, and is accompanied by 21 plates. Further, he was particularly interested in volcanoes, and in 1890 published a considerable work (399 pages) entitled "Characteristics of Volcanoes, with Contributions of Facts and Principles from the Hawaiian Islands," which placed on record much useful information collected by him during his travels in volcanic regions.

His zoological work, which was even more extensive than that in mineralogy and geology, chiefly related to the Wilkes expedition of 1838—1842. This expedition followed in part the course taken by the "Beagle," in 1831—1836, and even where it diverged from that route, visited coral and volcanic islands, such as have been carefully described by Charles Darwin. When the Wilkes expedition reached Sydney, in 1839, Dana chanced to see in a newspaper there a statement of Darwin's theory of the origin of the atoll and barrier forms of reefs. This statement, though very brief, was so suggestive as to be of great help to him in his work, and he ever afterwards expressed feelings of the deepest gratitude to its author. A visit to the Fiji Islands in 1840 (where two of his colleagues were murdered by the natives) brought before him facts such as had been noticed by Darwin elsewhere; but in the Fiji Islands they were on a still grander scale, and of a more diversified character, thus enabling him to speak even more positively of the theory than Darwin himself had thought it philosophical to do. One large volume of his expedition report was devoted to zoophytes, and described over 200 new species. His report on the crustacea occupied two volumes, and described 680 species, of which more than 500 were new to science. Each volume was illustrated by an atlas of beautiful plates, most of them drawn by himself. Though the last half-century has brought some slight changes to his classification of the crustacea, that of the corals stands to-day nearly as it was given by him in the report. This was the first time that any considerable number of the coral animals had been described and figured from life. Dana further showed that the reef-building corals live only in water having a temperature of at least 68° F., and that the distribution of the corals thus depends on the

temperature of the water. His expedition report on "Coral Reefs and Coral Islands," separately published in 1853, was afterwards extended by him into an elaborate account of "Corals and Coral Islands" (406 pages), the preface of which bears the date of 1872.

His work procured him many marks of distinction, both at home and abroad. He was an original member of the National Academy of Sciences of the United States, and in the year 1854 occupied the presidential chair of the American Association for the Advancement of Science. He was made LL.D. by Amherst College in 1853, and by Harvard and Edinburgh Universities in 1886. In 1851 he was elected a Foreign Member of the Geological Society of London, and in 1872 received from that society its Wollaston Medal. In the same year the University of Munich conferred upon him the degree of Ph.D.; in 1877 he received the Copley Medal of the Royal Society of London, and in 1884 was elected one of its Foreign Members. He was a corresponding member of the scientific academies of Paris, Berlin, Vienna and St. Petersburg, and of many other learned societies.

During all his life he had a strong love for music. He played the violin and the flute, was for a time the leader of the college choir, and even made some attempts at musical composition. As a man he was noted for the gentleness and kindliness of his character, and was thus on excellent terms with his colleagues. Professor Dana leaves a widow and four children.

Owing to the specialisation rendered necessary by the progress of knowledge, it will be long before we can expect to have with us a man of science with attainments so varied in character as were those of the United States veteran, James Dwight Dana.

L. F.

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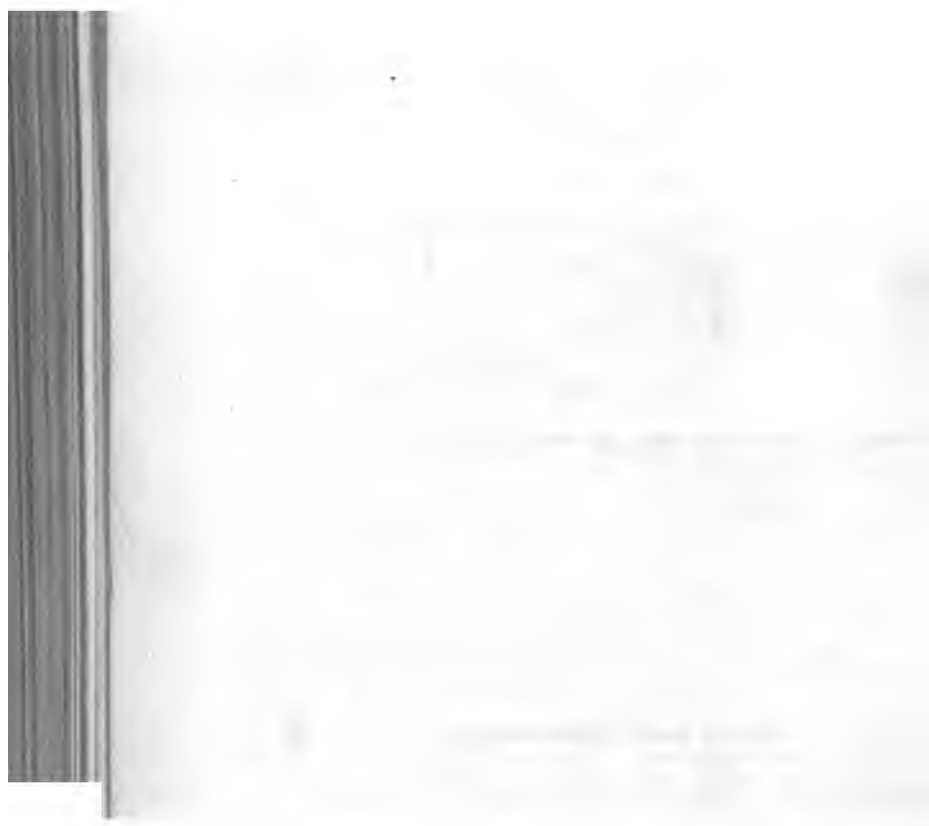
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On page 369, in the figure, *for* the right-hand letter B, *read* E.  
 On page 370, *for* Eg *read* HC.  
       "      " ABEC " AD'HE.  
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Page 239, line 18 from bottom, *for* ether *read* absolute alcohol.

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